PHILOSOPHICAL

TRANSACTIONS

OF THE

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FOR THE YEAR MDCCCXXXII.

PART II.

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Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

PHILOSOPHICAL TRANSACTIONS.

IX. *Some Account of a new Volcano in the Mediterranean.* By JOHN DAVY, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

Read December 22, 1831.

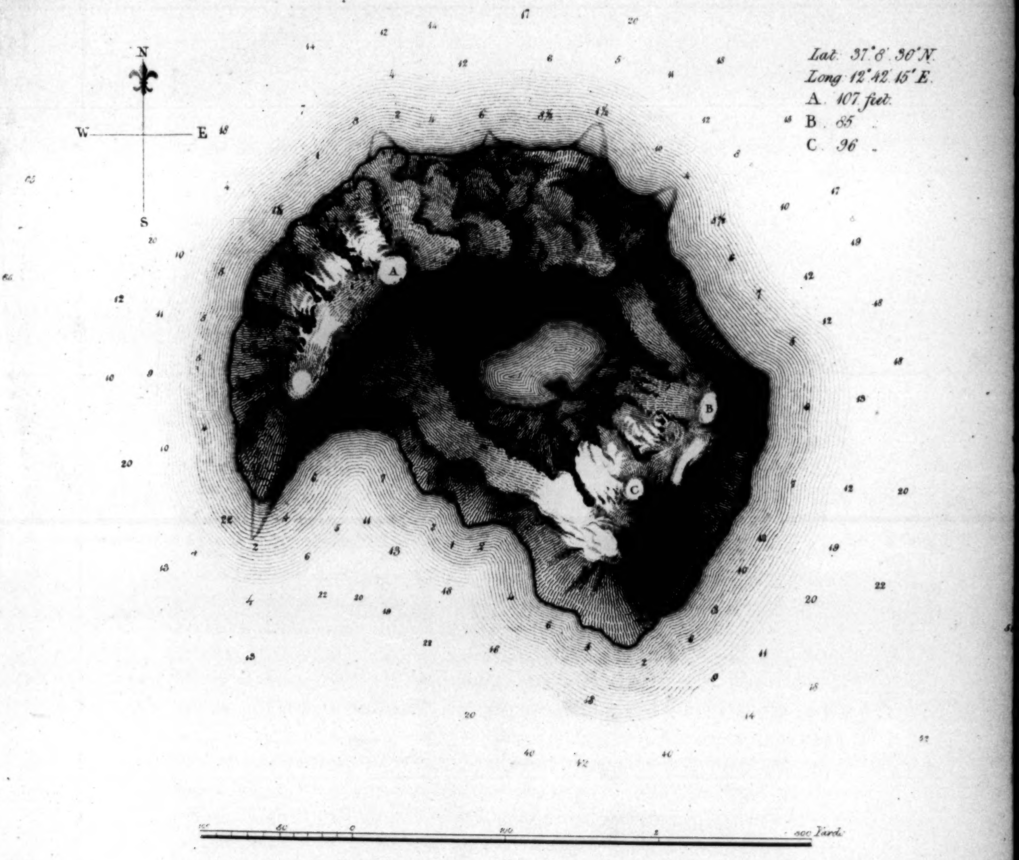
IN this communication I shall have the honour of laying before the Royal Society the information I have been able to collect respecting the volcano, which, three months ago, made its appearance off the southern shore of Sicily.

The first intelligence of its breaking out was brought to Malta, on the 16th of July, by a merchant vessel, the master of which stated, that on the 13th of that month, when passing between Sicily and the island Pantallaria, he witnessed columns of smoke rising from the sea, accompanied with a great noise, about twenty-five miles to the southward of Schiacca. The correctness of this report was confirmed by that of others, and all doubts that might exist respecting the nature of the phenomena were soon removed by the arrival in port of Captain SWINBURNE commanding HIS MAJESTY's sloop Rapid.

From the interesting statement of this officer, which was published in the Malta Gazette, it appears that on the 19th of July, when he succeeded in approaching very near the volcano, its crater was raised only a few feet above the level of the sea; that it was then in great activity, emitting vast volumes of steam, ashes, and cinders, without flame or red-hot matter; and that there was a constant flux and reflux of the sea by a breach in its side. Captain SWINBURNE remarks, that when passing the same spot nearly a month before, namely on the 28th of June, several shocks of an earthquake were felt; so there can be no doubt that the cause which produced the eruption was then in operation.

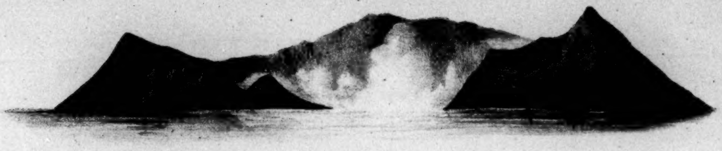
From the 19th of July till the 16th of August, the volcano continued active, and was gradually enlarged in all its dimensions. Partly owing to the curiosity of individuals, but chiefly to the watchful care of Vice-Admiral Sir Henry Hotham, during this period it was almost constantly under observation. Its activity appears to have been greatest about the 7th of August, when it was visited by Captain Irton, of the 2nd batallion of the Rifle Brigade, who made a very successful series of sketches and drawings, exhibiting its different appearances, both at rest and in action; copies of some of which, through his kindness, are annexed. At this period it emitted more ignited matter than previously or afterwards, but even then its fire was rarely distinctly visible by day. Its eruptions, it may be inferred, ceased suddenly, for on the night of the 15th of August it was seen in a state of considerable activity by a party of officers of the 73rd regiment, and two days after, when visited by another party composed of officers of the Rifle Brigade, it was in a tranquil state, emitting merely steam or aqueous vapour. Since that time there has been no fresh eruption.

Whilst the eruptions lasted, there is reason to believe that the form and dimensions of the volcano were almost constantly varying, according to the quantity of ashes discharged, and the violence of the explosions. When in a state of rest, between the 20th of August and the 3rd of September, during a period of fine weather, it was carefully examined by Captain Wodehouse, R.N. commanding His Majesty's brig Ferret, who landed on it repeatedly and ascertained its exact dimensions. He has been so obliging as to favour me with a plan of it, taken from actual survey, a copy of which, with his permission, I shall attach. (Plates V. and VI). According to this survey, the circumference of the island was about 3240 feet, and its greatest height 107 feet, and the circumference of the crater was about 780 feet. He found the surface tolerably cool, and composed entirely of ashes and cinders without any lava. The crater contained turbid salt water of 200° Fahr., from which, besides aqueous vapour, there was a constant disengagement of gas. He had specimens of the ashes and scoriæ collected, and also of the water and gas, which he did me the favour of sending to me on his arrival at Malta, and which I shall revert to hereafter. As far as the sounding-lead could be thrown into the crater, the water was very shallow, not exceeding three or four feet; and the crater was evidently filling up rapidly, by the falling in of its margin.

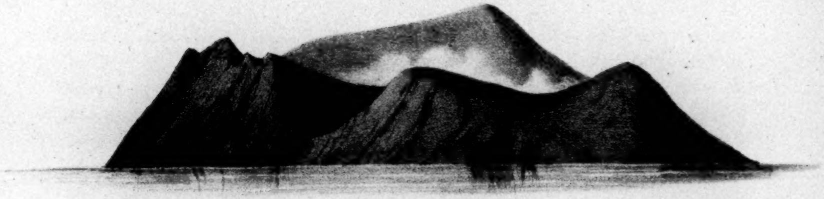


GRAHAM ISLAND.

C. Hullmandel's Lithography



Bearing N.N.E. distant 2 miles.



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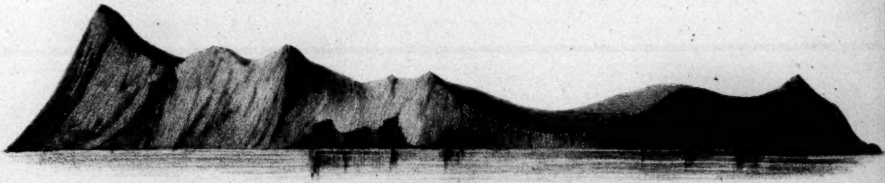

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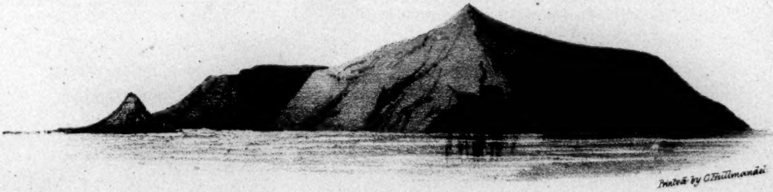

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N. W. 1 mile.

GRAHAM ISLAND

August 7th 1832.

This is a very brief sketch of the progress and present state of the volcano. From its commencement till now, I have not been able to ascertain that anything remarkable has occurred in the adjoining volcanic regions. At Schiacca, it was stated that the hot sulphureous springs at one time had become cold, and at another had ceased to flow; but I believe this was merely idle rumour; the truth of it has been denied by persons who had opportunities of being accurately informed. It was also stated, that at the commencement of the appearance of the volcano, Etna was more active than usual, and that severe earthquakes were felt at Catania. The occurrence of shocks of earthquakes has been confirmed, but not the increased activity of the mountain; on the contrary, I have been assured that it then emitted less than its ordinary quantity of smoke. The volcano of Stromboli, I have been informed, exhibited its usual appearance, and nothing uncommon is reported to have occurred in any other of the Lipari Islands.

Reflecting on the peculiar situation of the new volcano, many miles distant from land, and rising out of a comparatively deep sea, I indulged in the hope that, by a careful examination of the phenomena of its eruption, some information of a satisfactory kind, either positive or negative, might be obtained respecting the cause to which it owed its origin, and respecting the causes of volcanos generally.

I shall now give the observations which I made, with this object in view, on the 5th of August, when, through the kindness of Captain WODEHOUSE, I visited the volcano in the vessel under his command. The volcano that day was in a state favourable for the purpose; it was rather more active than usual; dense white vapour was constantly rising from it, and, at uncertain periods, about every two or three hours, explosions took place, and immense volumes of white vapour, mixed with, and sometimes obscured by ashes and cinders, were thrown out and projected to a great height, but without any appearance of fire.

To observe the phenomena more closely, we quitted the brig, which lay-to about two or three miles off, and proceeded towards the volcano in a boat. This was about ten o'clock A.M., when the volcano was most active. We first approached it to windward, to have an opportunity of observing narrowly the appearances. The wind being fresh, the ashes and cinders fell principally on the other side, in which direction the smoke and vapour were driven. The sea, within two or three hundred yards of the volcano, was clear and transparent, and of the usual dark blue of the Mediterranean. Nowhere could I observe any ascent or bubbling of gas in the water; nor did we feel any shock or commotion when eruptions took place. The appearances of the eruptions were almost constantly varying, according to the nature and quantity of the matters thrown out. The most common appearance was that of dense white vapour, resembling snow or bleached wool, which, thrown up in continuous masses, rose to a great height, and assumed various extraordinary forms. This effect, in all probability, was chiefly owing to the vapour of water. At first, from its great density, I was disposed to believe that the vapour might contain muriatic acid or muriate of ammonia, or the hydrated boracic or fluo-boracic acid; but none of the products which I afterwards examined were favourable to this idea. When I watched carefully a cloud of this vapour, floating before the wind, it gradually dissolved, and at last entirely disappeared, with the exception of a very faint, just perceptible, gray vestige, which was probably very fine dust, and perhaps saline matter derived from salt water. When the eruptions were most violent, the white vapour was followed by columns of perfectly black matter, which sometimes rose to the height of, perhaps, three or four thousand feet, and spread out very widely, even to windward. Once or twice there was an appearance of lurid fire. When the eruptions were of moderate strength, the columns of black or brown matter, intermixing with the masses of white vapour, or ascending through them, produced appearances very novel and impressive. The sounds attending the eruptions were not very loud; they resembled more the rumbling of heavy carriages on a pavement, than the reports of explosions. The thunder, produced by the lightning, which was almost constantly darting in various directions in the atmosphere of the eruption, much exceeded the subterraneous sounds in intensity. I watched, when the lightning was most vivid and the eruption of the greatest degree of violence, to see if there was any inflammation occasioned by this natural electric spark,—any indication of the presence of inflammable gas; but in vain.

Having satisfied our curiosity on this side, we proceeded towards the other, skirting the margin of the dense clouds of vapour and ashes which descended and spread over the surface of the sea. In passing, we saw the breach, through which there appeared to be a current constantly setting into the crater, and the water on the outside in an apparent state of ebullition. To leeward the sea was very much discoloured, and rendered turbid by ashes and dust; and cinders in plenty, of a very light kind, were floating on the surface. To ascertain if there was any peculiar smell belonging to the eruption, we passed a little within the skirts of the cloud, and the wind then freshening, we found ourselves in the midst of a dark shower of ashes, which fell with the force of fine hail, covered our persons, and almost blinded us. It was not in the slightest degree heated; indeed the wind that brought it, and which appeared to come from the atmosphere of the volcano, was unusually cool. The dust was quite dry, and some that collected on the folds of our dress had a strong saline taste: I shall revert to it again in considering the chemical nature of the products. Excepting once or twice when we perceived a slight smell of sulphur, no unusual odour, not the slightest bituminous smell, or smell of sulphuretted hydrogen, or of sulphurous acid, or of any other acid fume was observable.

Shortly after we had pulled out of this cloud, the volcano became quiet; and, the wind dispersing the vapour, the island appeared unobscured. We were so near to it, that it appeared practicable to reach it, and procure some specimens of the matter of which it was composed. When we were within about a boat's length of its precipitous shore, Captain Wodehouse ordered soundings to be taken; and it was found that the depth of the water was eight fathoms. Whilst part of the boat's crew were engaged in pulling in the lead, we had warning of an eruption by a rumbling subterraneous sound, immediately followed by the projection of a column of vapour, and which in a few seconds was succeeded by an eruption of ashes and cinders. The larger and heavier masses passed over us, and fell at a distance. For about half a minute, we were nearly in complete darkness, owing to the thickness of the dust and ashes ejected. I held my breath as long as possible, not expecting that the vapour would have been respirable; but, when obliged to breathe, I found no inconvenience from it, nor did Captain Wodehouse, or any of the boat's crew. For a moment I felt a hot blast; but this was very partial, and was not perceived by any one else in the boat. There was no unpleasant smell or acid fume of any kind. The eruption was slight, and of short duration;

in a few minutes the vapour was dispersed by the wind, so that we were able to see, and hasten to a distance. We found ourselves completely wetted with salt vapour or spray, and covered with wet ashes, of which I had no difficulty in collecting a sufficient quantity for examination. After this we returned on board.

Both in coming from the brig and nearing the volcano, and in returning, I paid attention to the temperature of the sea, and ascertained it by the thermometer. At 10 A.M., when we entered the boat, at the distance of two or three miles from the volcano, the sea at the surface was 80°, which is about the average temperature of this part of the Mediterranean in the month of August. To windward, as we approached the volcano, the temperature of the surface varied from 79° to 78°; to leeward, it was lower; when within about twenty yards of the volcano, it fell to 70°; and when nearest, within six or eight yards, it was 72°. On leaving it, the temperature gradually rose; when about a mile from it, to leeward, and still in turbid water, it was 76°; a little beyond this the water suddenly became clear, and the thermometer immersed in it rose to 79°. This low temperature of the water, close to an active volcano, is not what might be expected at first, and it appears paradoxical. It is probably owing to one or both of two things; either to the fall of cinders and ashes into the sea, projected so high as to be cooled in their ascent, bringing down with them the low temperature of the upper air; or, to the concussions from the eruptions throwing up cold water from the bottom of the sea. This latter supposition is so much the more probable, as there was a pretty rapid current flowing by the island at the time, the necessary effect of which must have been to prevent the accumulation of heat.

The whole of the night of the 5th we remained off the volcano, and in the evening and the early part of the night we witnessed some considerable eruptions. The reports attending them were much louder than in the morning; some of them resembling the reports of heavy artillery, and others the discharge of muskets; these latter were solitary, and occurred at intervals. The fire was very distinct in the darkness; but even when brightest, the ashes and cinders thrown up seldom exceeded a dull red heat. Twice or thrice I saw small masses shoot up, of a glowing white heat; but I was doubtful, at the moment, whether the effect was from electrical light or ignited matter. As in the morning, I watched carefully for the appearance of flame, but could not detect it; the lightning traversed in various directions the volcanic atmosphere, but it was never accompanied by any appearance of the explosion of inflammable gases.

The results of the preceding observations are all of a negative kind. Still hoping to ascertain something positive relative to the cause of the phenomena, after my return I availed myself of every opportunity of examining the productions of the volcano, and through the kindness of several friends I have been liberally supplied with materials.

The solid products, or matters ejected, which I have examined, have appeared to differ more in form than in chemical composition. They have occurred in the form of fine sand or ashes, of very porous light cinders, of comparatively heavy and compact cinders, and of fragments of vesicular lava; of the last variety of product, I have seen only two small specimens, which were taken from the crater on the 2nd of August, by Captain SENHOUSE, R.N., who was the first who succeeded in landing on it, and who has proposed for it the name of "Graham Island." Both these masses were of a dark gray colour, contained augite, and very much resembled vesicular basalt, or the common lava of Etna and Vesuvius, such as is quarried at Portici and at Catania. The specific gravity of one of them was 2·07; that of the other, which was more compact, 2·70. The very light, spongy cinder, which abounded floating on the sea, varied in colour between black and gray. Reduced to fine powder by trituration, and the greater part of the entangled air got rid of, it sunk in water, and was found to be of the specific gravity 2·64. The fine sand or ashes, which fell in our boat when we were in a shower of it, was of the specific gravity 2·66, and some which fell on us in the eruption, close to the volcano, 2·75. The cinders, of which the crater appears principally to consist, are commonly of a dark colour, and almost black, and they are generally very porous or spongy. Occasionally they are coloured superficially by yellow ochre, or a crust of clay mixed with a little peroxide of iron. One specimen, reduced to powder, was of the specific gravity 2·74.

Every specimen of solid matter that I have examined has contained saline matter similar to that of the sea, and a slight trace of sulphur. In every specimen tried, reduced to fine powder, there were particles which were attracted by the magnet. None effervesced with acids; all were readily fusible before the blowpipe, and ran into a black or dark green glass. I could not detect in any of them the smallest trace of carbonaceous matter, or any free acid, or alkali, or uncombined alkaline earth. From experiments which I made on small portions of each kind, they all appeared to consist of alumine, lime, magnesia and silex coloured by protoxide of iron, and without any potash. The absence of crystalline structure was very remarkable in all of them, with the exception of the small masses, already alluded to, of vesicular lava *.

I have already mentioned that I was indebted to Captain WODEHOUSE for a specimen of the water of the crater, taken up soon after it had become tranquil, and when its temperature was 200°. He furnished me with three wine-bottles full,—one of them from that part of the crater which was almost separated from the main crater by a bar of cinders, and was called the "small crater," and two from the main crater. They were well secured with corks.

The specific gravity of the water from the "small crater" was 1·057; that of one from the main crater was 1·069, and that of the other 1·070. In properties and composition they appeared to be very similar. They were free from any odour, of a dirty fawn colour, from a fine dust which was suspended in them, and which on rest subsided; after which they became perfectly clear and colourless.

The sediment obtained by filtering the water from the main crater (about three pints) weighed thirty grains. It consisted of a light brown ochrey pow-

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* Since writing the above, I have been favoured by Captain SENHOUSE with four specimens of rock ejected by the volcano, which from their nature it may be inferred were thrown up from the bed of the sea. Three of these are water-worn pebbles, different varieties of limestone; one of them is highly crystalline dolomite, containing a considerable quantity of magnesia; another is finely crystalline, and contains a smaller proportion of magnesia; and the third is of very fine grain, not crystalline, with only a trace of magnesia. The fourth specimen, which is a fragment of a mass said to have been of several pounds weight, has a good deal of the character of graywacke. It contains, disseminated through it, in a solid state, saline matter, chiefly common salt. It effervesces with acids, and gelatinizes. From the few experiments I have made on it, it appears to be composed of a large proportion of silica, and of lime, magnesia, and alumine in about equal proportions, and to be coloured by protoxide of iron. Whether it contains any lime or magnesia not combined with carbonic acid, I have not ascertained. It is of considerable hardness and toughness, and is infusible before the blowpipe. Portions of its surface are covered with a vitreous fusible scoria, similar to that of the volcano, as if it had passed through, or come in contact with the fused matter of the volcano.

der, of a fine blackish dust, and of fibres, in appearance not unlike vegetable fibres. No carbonate or sulphate of lime could be detected in it, and only a very slight trace of sulphur. The dust and powder were very fine volcanic dust; the black, as ejected; the yellowish brown coloured by peroxide of iron, instead of the protoxide, probably from the action of the atmosphere on the latter. The fibres resembling vegetable fibres consumed before the blowpipe, with a smell very like that of sea-weed burning; and it may be conjectured that they were derived from sea-weed drawn into the crater. The same kind of fibres, it may be remarked, were frequently to be seen on specimens of cinders brought from the volcano; and their origin, it may be supposed, was the same.

The water from the "small crater" after the separation of its sediment, evaporated to dryness with great care over boiling water, afforded 8·6 per cent. saline matter. The water from the main crater (the two bottles mixed) similarly treated, afforded 10·6 per cent.

From the experiments which I have made on these specimens of water, they appear to differ chiefly from the water of the Mediterranean, not in their principal saline ingredients, but in containing more sulphate of lime, and a little alumine, oxide of iron, and a trace of oxide of manganese, all three in combination with an acid, probably the sulphuric and muriatic,—and a notable portion of hyposulphite of lime and magnesia. I could not detect in either of them any free acid or alkali, or the presence, even in combination, of any potash, ammonia, or nitric acid; nor the slightest trace of bromine or iodine. In quest of these latter substances, 77 cubic inches of the water from the main crater were carefully evaporated, and the greater part of the whole from the "small crater"; and the most approved tests, as recommended by M. Balard, were applied to the deliquescent salts extracted by alcohol, without the slightest indication appearing of either of them. A solution of chlorine very carefully dropped into the concentrated saline solution occasioned no discoloration; and the starch solution did not produce any tint of blue, whether the chlorine was used alone, or added to the salt with excess of sulphuric acid.

For comparison, I took up from the sea, in returning from the volcano, six different specimens of the water of the Mediterranean. No. 1. was taken up about forty yards from the volcano, and was slightly turbid. No. 2, about three miles from it, where the sea was clear. No. 3, about five miles distant.

No 4, about three miles from Cape Bianco, in the neighbourhood of Girgenti. No. 5, between Girgenti and Gozo, in lat. 36° 33′, long. 13° 31′. And No. 6, about a mile off Gozo. I ascertained their specific gravity with great care, by means of a delicate balance of Robinson's, and found it the same in each instance, viz. 1·0287, water at 75° Fahr. being 1·0000, which is about the average specific gravity of the surface water of the open parts of the Mediterranean in the summer season *. I ascertained also with care, the saline residue which each specimen afforded, evaporated over boiling water and exposed to this temperature as long as any loss was sustained. The evaporation was made in a silver capsule, which was weighed as speedily as possible and whilst still warm. The residue per cent. of each was as follows:

No. 1	4·46
2	4·43
3	4·40
4	4·39
5	4·43
6	4·33

Even these slight differences of the quantity of saline residue might have been, and probably were owing to the circumstances of manipulation, and the state of the atmosphere in relation to humidity when the experiments were made. In all of them were discoverable a slight trace of sulphur and an extremely minute quantity of iodine. The apparent absence of the latter substance in the water of the crater, might either have been owing to the high temperature to which it had been exposed, and to which its superior specific gravity is to be attributed, or to the presence of the hyposulphites, which perhaps might have masked such a very minute quantity, if present.

I have now mentioned all the products of the volcano that have come to my knowledge, excepting the gaseous. The specimens of gas (two in number) which I received from Captain Wodehouse were in the wine-bottles in which they had been collected. One was full of air; the other contained about four fifths air and one fifth turbid water of the volcano. They had merely been

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* In bays in which no rivers empty themselves, I have found the specific gravity of the water higher; and towards the embouchures of great rivers lower, as in the Adriatic and the Hellespont.

corked, and had not been preserved with any of the precautions requisite to have prevented the escape of some of their gaseous contents and the admission of atmospheric air. As soon as I received them (it was at night), they were inverted in water, and the following morning they were examined. In the first-mentioned bottle, which had been full of air, a little water had entered so as to fill about half its neck. On withdrawing the cork under water, water rushed in equal to about one quarter of the capacity of the bottle. The air remaining had a slight smell of sulphuretted hydrogen; it extinguished a taper plunged into it, and was not itself inflammable; 50 measures of it by lime-water were reduced to 16; and these by phosphorus were reduced to 13½; sulphur sublimed in this residue occasioned no alteration of volume.

The air in the other bottle containing some water had no smell of sulphuretted hydrogen; 48 measures of it by lime-water were reduced to 33, and these by phosphorus to 31; and this residue was not inflammable, and extinguished flame.

From these results it may be inferred that the gas in the first-mentioned bottle consisted chiefly of carbonic acid and azote and a little oxygen with a trace of sulphuretted hydrogen; and that the gas in the second bottle was principally azote with a little oxygen and carbonic acid. Considering the manner in which the bottles had been kept, it is highly probable that the azote and oxygen were derived from atmospheric air, unconnected with the volcano, and that the carbonic acid and trace of sulphuretted hydrogen alone were of volcanic origin. The presence of the acid gas is easily accounted for, supposing it to be derived by the action of heat from rocks containing carbonate of lime and magnesia, earths which we have seen are contained in the cinders and ashes ejected. In one place outside of the volcano, in the sea, Captain WODEHOUSE observed a great bubbling of air, as if the water was boiling; he approached it and even went over it in a boat, and found its temperature not above that of the adjoining surface, and there was no peculiar odour perceptible. It is to be regretted that none of this gas was collected; but, probably, it also was carbonic acid gas, and arising from the calcining effect of heat on the subjacent rocks forming the bed of the sea. Shortly after, it is said, the bubbling continuing, the water became very hot, which is confirmatory of the above conjecture.

I have stated already, that whilst I was at the volcano, no indications appeared of the disengagement of any inflammable gas, and not even of any acid gas or vapour. I have conversed with many gentlemen on whose accuracy of observation I could place dependence, and their experience agreed with mine, excepting that one or two of them perceived distinctly acid fumes, which, from the description given of their effects when respired, it may be inferred were of sulphureous acid. Probably a little sulphuretted hydrogen also was evolved; but it must have been in extremely minute quantity, otherwise it could not have escaped notice. In the gas I examined, the trace of it was so slight that it was not discoverable by means of freshly precipitated oxide of lead; a few particles of it agitated with the gas, were not in the slightest degree discoloured.

In an account of the volcano, published in the Malta Gazette of the 25th August, it is stated that carburetted hydrogen was evolved from it, and that coal deprived of bitumen occurred amongst the ashes and scoriæ. As the writer does not appear to have ascertained either of these points in the only way in which they could be determined in a satisfactory manner, namely by experiment, I am under the necessity of supposing that his statement in these particulars is not correct, and that the appearances to which he trusted were fallacious.

The results of my latter inquiries, it will be perceived, like those which preceded them, are entirely negative; and they are very similar to those which my brother, the late Sir Humphry Davy, obtained at Vesuvius, which he has described in a paper, "On the Phenomena of Volcanos," published in the Philosophical Transactions for 1828; and reasoning on them in relation to the theory of volcanos in general, they appear very favourable to that hypothesis of volcanic action to which he gave the preference, both in the paper just alluded to, and still more decidedly in his posthumous work, "Consolations in Travel;" namely, the simple hypothesis of an ignited nucleus of fused matter, occasionally forced through the cooled crust of the earth by the expansive power of steam and gas. In the present instance, all the phenomena and circumstances of the volcano happily accord with this view. The situation of the eruption, many miles distant from the nearest shore, seems to be incompatible with its having any connexion with the atmosphere; and this idea is supported by the depth of the sea where the volcano appeared,—a depth which, according to the most accurate survey, must have been at least 50 or 60 fathoms. Further, the products examined, whether solid or gaseous, may be said to demonstrate that ordinary combustion was nowise concerned in the phenomena; and the absence of inflammable gas in any efficient quantity, (of which it appears to me no doubt can be entertained,) seems no less forcibly to demonstrate, that the decomposition of water by the metallic bases of the earths and alkalies, cannot be admitted as the principal cause. On the other hand, if we suppose a state of things in conformity with the hypothesis of our globe having been once in fusion, and being still so at a certain depth beneath the surface, liable to be acted upon by water flowing in from above, the phenomena of the volcano do not seem to be of difficult explanation; they are indeed such as might be expected à priori; namely, the vast quantity of aqueous vapour evolved impregnated with salt; the porous cinders and ashes ejected; the comparatively low temperature of the ejected matters, and the apparent absence of any gas in considerable quantity, excepting carbonic acid. All the other observations which I have made at different times in the volcanic regions of Italy, Sicily and the Lipari Islands have been of the same negative character as the preceding, and favourable to the same hypothesis rather than to that of the chemical origin of volcanos. The subject however is so mysterious, that what is probable, on further inquiry may not prove true; and other causes may be discovered, which at present are not even imagined.

Malta, October 25th 1831.

Explanation of the Plates.

Plate V.

A plan of the island, with soundings from a survey by Captain Wodehouse. Its outline and that of the crater is from observation. The ground has been sketched in by Captain Irton, partly from views which accompanied the original plan, and partly on supposition.

Plate VI.

Fig. 1—4.—Profile views of the volcanic island as it appeared on the 7th August.

X. *Further Notice of the New Volcano in the Mediterranean.* *By* John Davy, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

Read March 15, 1832.

THE last communication I had the honour to make to the Royal Society on this subject was dated the 25th October. Since that time the crater of the volcano, from the operation of various causes, has undergone several changes of form, and now it has disappeared entirely. Of these mere changes of form I shall not attempt to give any description, as they have not been minutely observed, and as no inference of any importance, that I am aware of, is to be drawn from them, excepting that the crater was one of "eruption," composed entirely of loose materials thrown up by volcanic action.

I notice this inference, because, in some accounts of the volcano which have appeared in the newspapers, it has been asserted that the crater was decidedly one of "elevation," that is, formed of rock once composing the bed of the sea, which had been elevated by volcanic force acting from below previous to the eruption. How such an opinion could have arisen, it is not easy to conjecture; I am not acquainted with a single circumstance connected with the crater that is favourable to it.

From the reports of masters of vessels, which seem deserving of credit, the crater disappeared in the latter end of December. About that time there were strong gales, a tempestuous sea, and very heavy rains; and, considering its composition, these causes seem adequate to account for its destruction. Its situation is now only marked by a dangerous shoal, on which from the latest accounts there are only a few feet of water.

In reply to some queries which a gentlemen of Malta was so obliging as to take with him to Sicily on a visit to the southern part of the island nearest to the volcano, I have been informed that its smoke or vapour was first seen from the shore on the 11th July; that a few days previous, two or three slight shocks of an earthquake were felt along the coast from Sciacca to Marsala; that about a fortnight after, the air became dark and loaded with vapours, which at Sciacca had a distinct sulphureous smell; that the noise of the explosions was sometimes heard as far as Mazzara; and lastly, that the baths of Sciacca were a little hotter than usual.

These are all the additional particulars I have been able to collect which are deserving of credit. I have seen some fresh specimens brought from the volcano since my first account was drawn up; but they have proved, on examination, so very similar to those described in it, that they do not require particular notice. It may be, perhaps, not undeserving of mention, that two or three pretty large masses of vesicular lava were found amongst the loose ashes and cinders of the crater. The largest that I have seen or heard of weighed twenty-seven pounds; it was in the possession of Captain SENHOUSE, and resembled exactly the small fragments which I received from him, and which I have already noticed. Its appearance indicated that it had been thrown up in a solid state, after its angles had been worn like those of water-worn stones. Whether it is to be considered as a water-worn stone analogous to the dolomite pebbles alluded to in my paper, previously existing at the bottom of the sea, or of recent formation in the interior of the crater, or detached from an old bed of lava and worn by attrition during the eruption, it is difficult to decide.

When a remarkable phenomenon occurs, anything unusual happening at the same time is apt to be attributed to it, especially if there is any kind of analogy between them. The last summer in Malta was unusually hot; the thermometer exposed to the wind, more than once rose to 105° of FAHRENHEIT; this was generally supposed to be owing to the volcano. In the month of August a singular appearance was witnessed in the heavens, many evenings successively, both here and in Sicily; soon after sunset the western sky became of a dark lurid red, which extended almost to the zenith, and continued gradually diminishing in extent and intensity even beyond the limit of twilight. This phenomenon, too, was attributed to the volcano; and was supposed by many people, whom it greatly alarmed, to be portentous of some impending calamity, and especially of the invasion of the epidemic cholera. Whether this fiery sky and the great heat of summer were really connected with the volcano in the relation of cause and effect, it may be difficult to determine; but I am rather disposed to consider them independent of it, especially the latter, as the hottest wind during the summer was from a different quarter, as the volcano emitted comparatively little fire, and as the temperature of the atmosphere in its immediate vicinity was very little affected by it.

Malta, January 28th, 1832.

XI. *Some Remarks on an Error respecting the Site and Origin of Graham Island. By Captain* W. H. Smyth, *R.N. F.R.S. F.S.A.*

Read February 9, 1832.

In consequence of accounts recently published concerning the rise and progress of this island, which I conceive to have been stated materially in error, and in order that physical inquiry may receive as exact data as can be afforded, I beg leave to offer the following remarks to the Royal Society.

It was stated, in the first letters which arrived from Malta, that an officer on the Mediterranean station was in possession of an old chart, whereon was "a shoal with only four fathoms on it, and called Larmour's Breakers;"—and this being asserted to be "within a mile of the latitude and longitude" of the new island, was consequently announced as its nucleus. On reading some of these letters I saw at once that the chart was mistaken for a valuable document; but being aware that its particulars were well known to navigators, I should not have deemed it to require notice, had not the erroneous inference been repeated, both in the Journal of the Geographical Society, and in the Quarterly Review.

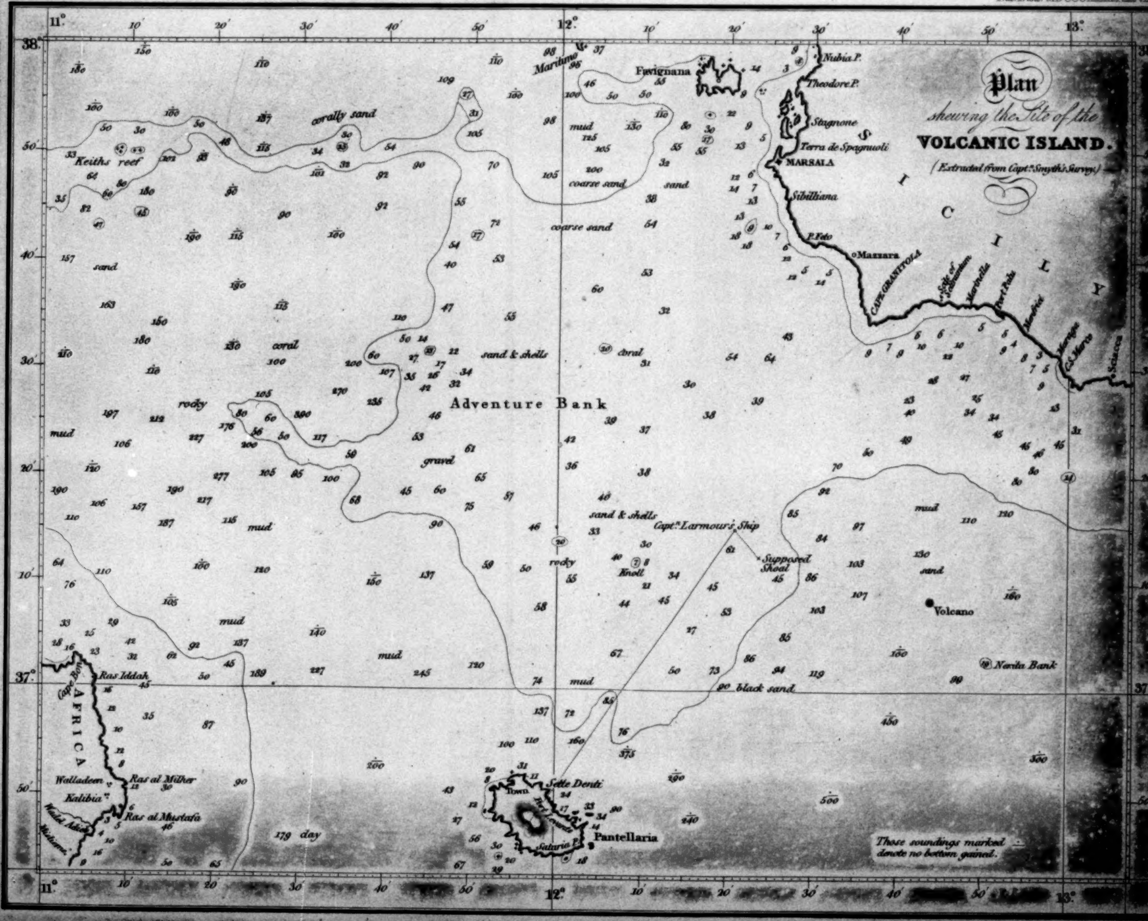
The danger alluded to as existing, upon the "old chart", was never ascertained or verified; it was only thought to have been seen, by Captain Larmour, when in command of the Wassanaer, a troop-ship, on the Egyptian expedition. But the same impression did not strike all the officers and passengers; and on the commander-in-chief dispatching two or three vessels to examine it for a more detailed report, no shoal-water could be found. The present Captain Richard Spencer, C.B., then a lieutenant on board the Wassanaer, was one of the officers sent to assist in the search; and from him I had these particulars. Yet the minute which had been forwarded to me from the Admiralty, being written in these decided terms—

> "H. M. Ship Wassanaer, 11th of December 1800, P. M. The island of Pantellaria S.W. by W. 9 or 10 leagues, saw a reef of rocks S.S.E., distant 3 or 4 miles, extending N.N.W. and S.S.E., about one mile in length. Hauled up S. by W., to clear them. Saw something on the reef like a ship's mast. Bearings by compass."—

I examined the spot with a rigorous strictness, (see Plate VII.); and from the various traverses which I made in every direction, with the lead going by night and by day, I feel prepared to assert that, no reef of the nature described by Captain LARMOUR in 1800, and no shoal of four fathoms water, could have existed in 1814. How the said "four fathoms" crept into our charts, is best known to the ship-chandlers who too long purveyed to the scientific wants of seamen; but from the absence of positive testimony, from the careful search made by order of Lord KEITH, from my own several cruizes, and from the material fact of its being in the high road which is annually beaten by hundreds of ships, it is not presuming greatly to say, that neither the one nor the other had any existence.

Nor is the assigned place "within a mile" of the position of the volcanic islet, though it may accidentally have been so marked upon the "sea-cards;" for it should be remembered that the true site even of the principal headlands around was not then decided. According to the minute just quoted, corrected for magnetic variation, LARMOUR's supposed reef is no less than sixteen miles W. by N. from it, on a part of the sub-aqueous plateau (which I named Adventure Bank) uniting Sicily to Africa by a succession of ridges,—about a spot where I found from 40 to 50 fathoms of water. Graham's Isle, however, is not upon this bank; it arose between it and a knoll some miles to the eastward, which, from a shell brought up by the arming, I called Nerita; and, if the observations which determine the latitude and longitude of the stranger as in 37° 08′ 25″ N. and 12° 43′ 50″ E. be correct, it must have been elevated through more than a hundred fathoms of water.

In thus doubting the actual existence of the Larmour Shoal, it is not my intention to dispute the appearance and disappearance of natural phenomena; nor that stupendous alterations may occur by the subsidence and uplifting of strata,—because an obstinate scepticism would be absurd, especially in a part of the globe where, to use a well-expressed Italian metaphor, the whole ground


Plan
shewing the Site of the
VOLCANIC ISLAND.
(Extracted from Captn. Smyth's Survey.)
SICILY
Nubia P.
Theodore P.
Stagnone
Terra de Spagnuoli
MARSALA
Sibilliana
P. Feto
Mazzara
CAPE GRANITOLA
Selle of Selinuntum
Mariuella
Port Palo
Menfrici
Maragne
C. S. Marco
Sciacca
Favignana
Maritimo
Keiths reef
corally sand
coarse sand
sand
mud
coral
rocky
gravel
sand & shells
Adventure Bank
Captn. Larmour's Ship
Supposed Shoal
Knoll
Volcano
Nerita Bank
black sand
clay
Pantellaria
Sette Denti
Town
Port Lemani
Salaria P.
AFRICA
Cape Bon
Ras Iddah
Walladeen
Ras al Milher
Kalibia
Ras al Mustafa
Wadel Adea
Misharree
Those soundings marked denote no bottom gained.
11°
10'
20'
30'
40'
50'
12°
13°
38°
37°

is "tremblingly alive." But it is reasonable and proper to question such rumours as have been made without due examination. In the instance before us, no endeavour was made to establish the truth by either shortening sail, lowering a boat, or even getting a cast of the lead; moreover, they were three or four miles from the supposed object, and opinions on board the Wassanaer were not at all unanimous. By similar indecision a teasing knot of perils has gained random insertion upon our charts, to the disquietude of sea commanders; but it is a fault which is fast disappearing, and it may be trusted that there are few officers who would not think themselves liable to the imputation of culpable carelessness, did they not seek to verify such "dangers" as they might accidentally encounter.

I do not think sub-aqueous volcanic explosions are of such rare occurrence as is generally supposed; and extremely sudden intumescence may arise from the expansion of an inferior lava bed. It is not at all improbable that gaseous fluids, and ejectamenta, may have been seen, before the accumulation of solid matter, protruded from the vent, was sufficient to form a crater of eruption. A volcanic apex may become visible, and again be quickly destroyed by trituration, the solution of mineral substances, and the repressive force of the column of water over the vent. Now, as there was a chance that something of the kind had occurred in the neighbourhood assigned to Larmour's reef,—breakers having been reported near the same spot by the Greyhound frigate, and shoals having been immemorially marked there under the names of La Ajuga, and B. Scoglio,—I laboriously explored the whole vicinity. In examining the chart which resulted from this undertaking, it will be found that a knoll, with only seven fathoms upon it, was discovered not far from the site of all these reports, and that the Adventure Bank extends from Sicily nearly to Pantellaria, where the water deepens at once from 76 fathoms to no bottom with 375 fathoms of line. A further inspection will show that the Phlegræan islands of Pantellaria and Linosa have been protruded from the greatest depths, where perhaps the fires found the least resistance.

All these considerations led me to suppose that though the reports were exceedingly vague, volcanic agency might still have given grounds for them. I therefore made particular inquiries, both in Sicily and Pantellaria, as to local earthquakes, and whether any volumes of smoke, ferilli or jets of flame, comminuted ashes, or other fragmentary ejectments, had been noticed in that direction; but I could hear of none. Yet we are told, as a "fact" of weight, that a tradition is current, which says, "A volcano existed in the same spot about the commencement of the last century." It would be difficult to say how this tradition was preserved amongst a people little given to letters; and I never, in my long residence and systematic researches at the above place, and in Malta, heard the slightest hint of it.

I am therefore led to the conclusion,—firstly, that no shoal or danger has lately existed in that channel, excepting only an occasional overfall in very heavy weather on the 7 fathom knoll where I anchored H. M. ship Adventure, and which is sufficiently near for bearings taken at random, and without suspicion of the existence of local attraction, to be placed in identity with the reports above mentioned. Secondly, that even if what Captain LARMOUR became persuaded he saw, was actually a temporary volcanic effect, it had no possible relation to breakers with "four fathoms" upon them. And it follows, that the assertion of Graham Island having been formed by the mere "lifting up" of such shoal, must be utterly destitute of foundation.

XII. *An Account of some Experiments and Observations on the Torpedo* (Raia Torpedo, *Linn.*) *By* John Davy, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

Read March 22, 1832.

In a paper published in the Philosophical Transactions for 1829, my brother, the late Sir Humphry Davy, has given an account of some experiments which he made on the torpedo for the purpose of ascertaining the nature of its electricity, whether it is of a peculiar kind or analogous to kinds already known. The results he obtained were altogether negative, and seemed to lead to the former conclusion. But that conclusion was so novel and important, that he did not consider himself justified in adopting it without further investigation. At the time he wrote the paper referred to, namely, in the autumn of 1828, in a very feeble state of health, he was on his way from southern Austria to Italy, where, if his health permitted, he intended renewing the inquiry. He arrived at Rome on the 19th of November, and, with his usual ardour of pursuit, immediately began his observations on the torpedo; but they were directed chiefly to its anatomical structure and natural history, rather than to its electricity; for, though this fish is to be had in abundance in the fish-market of that city, being brought from a distance, it is very difficult to obtain it alive. To make experiments on the living fish, he proposed going either to Civita Vecchia or Tormicina, where it is caught; but before he could accomplish this intention he suddenly experienced another and very severe attack of his complaint. This attack occurred on the 20th of February; and in a letter written from his dictation, five days after, when he considered himself dying, he particularly requested me to carry on the investigation; and such was his zeal for science, that, excepting in a postscript, no mention was made of the alarming state in which he then was. On my joining him from Malta, on the 16th of March, he was still dangerously ill, and had the same feeling of being near his end; but his mind was wonderfully clear and active, and his love of research as great as at any former period of his life. At his request, the following morning torpedos were obtained from the fish-market, and I amused him, day after day, with the results of my dissections, till his complaint acquiring an aggravated form, and threatening speedy dissolution, he was unable to attend to them. I then discontinued the inquiry, and till a few months ago, I have not had an opportunity of renewing it. The results which I have obtained I shall now have the honour of submitting to the Royal Society. The experiments which I shall first detail on the living fish have been made entirely at Malta, and under very advantageous circumstances; for, residing during the summer season close to the sea, I have been able to obtain torpedos fresh from the water, and in a state of great activity.

1. *Experiments on the Electricity of the Torpedo.*

My brother was very desirous of trying the effect of the shock of the torpedo on a needle placed in a spiral wire. The result, he was of opinion, would be conclusive as to the nature of its electricity. Anxious to make this trial, I had an apparatus in readiness, which, with common electricity, I had found to answer extremely well. It consisted of a fine copper spiral wire, about one inch and a half long, and one tenth of an inch in diameter, containing about one hundred and eighty convolutions, and weighing about four grains and a half. This was inserted into a glass tube, just large enough to receive it, and secured by corks. The wire passed through the cork at each end, and was connected with strong wires with glass handles for the purpose of contact. The wire which was intended to be applied to the under surface of the fish was one twenty-fifth of an inch in diameter; that intended for the upper surface was stiffer, being one fourteenth of an inch in diameter, and its greater strength was useful, as it was necessary to employ it occasionally with some force to rouse the fish when averse to give a shock.

The first trial I made with this apparatus was successful. The fish used was a small one, about six inches long; it had been just caught in a hand-net, and immediately put into salt water, and was very active. A needle, perfectly free from magnetism, was introduced into the spiral, and there confined by the corks, and the spiral was carefully connected with the insulated wires for contact. The fish for the experiment was placed in a glass basin, and was barely covered with water. One wire was applied to the under surface of the electrical organ, and the other to its upper surface, and contacts were made at intervals during about five minutes, when the fish seemed much exhausted by its exertions. On taking the needle out, and bringing it near some fine iron filings, it proved magnetic, and powerfully attracted them. This experiment I have repeated several times, with fishes of different sizes, some larger and others smaller, and with the same result, when the fish has been active and the contacts similarly made.

The next trial which I made of the electricity of the torpedo, was on the multiplier. The precaution was taken to insulate the instrument well, by smearing with sealing-wax the feet of the stand supporting the coil. The same wires for contact were used in this as in the former experiment, and the junctions were carefully made. Applying one wire to the under surface, and the other to the upper surface, with every fish which I tried I succeeded in obtaining decisive results; the needle by active fishes was generally thrown into violent motion, and even by the feeblest was distinctly affected. I have met with no instance of a fish which had the power of magnetising a needle in the spiral wire, failing to move the needle in the multiplier; but I have met with more than one example of a fish whose electricity was equal to the latter effect, and not to the former.

The experiments which I have instituted, with a view to ascertain if the electricity of the torpedo has any igniting power, or power of passing through air and producing light, have been attended with less satisfactory results. Very active fishes were tried on circles of perfect conductors, interrupted only by a space just visible with the aid of a powerful magnifier. The terminal wires, coated with sealing-wax, excepting at their extremities, were introduced through a perforated glass stopple into a small glass globe, which was held in the hand of an assistant. The contacts were made in the dark; but not the faintest spark could be perceived, nor could any ignition be perceived when the extreme points were connected by silver wire not exceeding one thousandth of an inch in diameter.

When a torpedo was put into a metallic vessel, insulated by a glass stand, and contacts were made on its back, with the insulated wire resting on the edge of the vessel, or at a distance from it, luminous appearances were frequently produced, sometimes in the form of sparks, and sometimes in the form of flashes, not unlike summer lightning on an infinitely minute scale. At first, I was disposed to consider the phenomena electrical; but, on reflection, it occurred that they might depend on the presence of animalcules, which became luminous when agitated. And this I believe is the correct explanation of the effects; for, when the salt water was agitated without the torpedo, sparks of light now and then were seen, and the flashes or coruscations might have been owing either to luminous matter thrown off from the surface of the fish when it gave a shock, or to the shock simultaneously stimulating several particles, which, in consequence, shone for an instant.

The only positive result which I have obtained on the passage of the electricity of the torpedo through air, has been by using a chain as a substitute for a wire of communication. It was a small gold chain, composed of sixty-six double links, each circular and about one tenth of an inch in diameter, fastened unstretched to a dry glass rod at each end. Holding the upper portion of this chain in one hand, and the under wire in the other, (the hands being moistened,) I irritated, by means of them, the upper and under surface of an active fish; the shock which it gave was pretty strong, reaching beyond the fingers, and was felt with the same degree of force in both hands. This seems to show that the air is not impermeable to the electricity of the torpedo; and the same conclusion may be drawn from the facility with which I have found it to pass through a circuit of wire in which there have been no less than seven joinings, and these made merely with ordinary care, with the fingers, without the aid of any instrument.

In accordance with Mr. Walsh and my brother, I have in no instance seen the torpedo affect the common electrometer, or exhibit any the slightest indications of a power of attraction and repulsion in air.

The experiments which I have made on it as a chemical agent have been of a satisfactory kind. A small glass globe, of the capacity of about half a cubic inch, was used for holding the fluid to be acted on; and fine wires, communicating with the contact-wires, were introduced into it through a perforated glass stopple, and they were coated with sealing-wax along their whole course in the vessel, excepting at their points. By means of this little apparatus, I first tried the effect of a small active fish on a strong solution of common salt; the terminal wires were of silver. The contacts were made on the upper and under surface of the fish in the usual manner; minute bubbles of air collected round the point communicating with the under wire, but none at the other point. After an interval of some hours, fine gold wires were substituted for the silver wires; now gas was evolved from each extremity, but in largest proportion, and in smallest bubbles, from the point connected with the under wire.

The next experiment was made on a strong solution of nitrate of silver; the terminal wires were of gold. The effect was distinct; the extremity of the under gold wire became black, and only two or three bubbles of air arose from it; the extremity of the upper gold wire remained bright, and it was surrounded with many bubbles of air. A similar experiment was made on a strong solution of superacetate of lead, and with results which were similar; but the effects appeared to be produced with greater difficulty; they were not distinct till the fish had been much irritated, and seemed to put forth all its energy.

Mr. Walsh inferred from his experiments, that the two sides of the torpedo are in opposite electrical states*. The results just described appear to prove that its under surface corresponds to the zinc extremity of a voltaic battery, and its upper surface to the copper extremity.

To ascertain if they preserve the same relation to each other when the fish is made to act on the multiplier, and on the needle in the spiral, the following experiments were made. Successively at different times with the same fish, and also with different torpedos, comparative experiments were tried on the course of the needle in the multiplier when affected by the electricity of the fish, and by that of a couple of very small plates of copper and zinc immersed in a weak acid. In every instance, the wire communicating with the under surface of the torpedo was found to correspond in its effect with the zinc plate, and that with the upper surface with the copper plate; and whether one wire was in communication with the under surface of the fish, and the other with the upper, or the former with the zinc plate, and the latter with the copper plate, the deviation of the needle was in the same direction; its south pole turned to the east, and, of course, its north to the west: and if the lower contact wire was made the upper, the effect on the deviation of the needle was identical with a change of the plates.

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* Philosophical Transactions abridged, vol. xiii. p. 475.

I have found the same uniformity of result in the polarity imparted by the torpedo to a needle in the spiral wire; the extremity of it, nearest the under surface in the circle, has always acquired southern polarity, and the other extremity, of course, northern.

By connecting the spiral with the multiplier, and charging the former with as many small needles as it could hold, namely, eight, I ascertained that a single discharge of the electricity of an active fish moved the needle in the multiplier powerfully, and converted all the needles into magnets; and each of them I believe was as strong as if one only had been used.

Using two spirals charged with needles, one connected with one end of the multiplier, and the other with the other end, the effects of the discharge were similar to the preceding, both on the needle of the multiplier and on the needles in the spirals. In two instances, the needles in the spiral connected with the upper surface, were most powerfully magnetised; and in one instance, the effect was greatest on the needles in the lower spiral. In this last instance nine needles were acted on in the under spiral, and six in the upper; the fish which produced the effect, with one exception, was the smallest that I had ever used.

The preceding are the principal experiments which I have made on the electricity of the torpedo, using perfect conductors to convey it. I have, besides, instituted some in which the communication by perfect conductors was interrupted by imperfect ones; a few of these I shall briefly notice.

When I have held the contact-wires in the palm of each hand, wetted with salt water, and have touched with the fore-fingers the upper and under surface of a torpedo, I have felt its shocks distinctly; but in no instance when the multiplier has been connected with the wires, has it been affected; and when the spirals have been connected with them, I have once only seen the needles in them converted into magnets. This effect accompanied a very smart shock from a young active fish, about six inches long, just taken.

When the touching ends of the contact-wires have been covered with leather soaked in salt water, or with cotton thread, all the effects of the fish, as might be expected, were witnessed, as if these imperfect conductors had not intervened; the shock was felt by the hands holding the wires; needles in the spirals were magnetised, and the multiplier was moved.

When a cotton thread, soaked in salt water, or in a strong solution of salt, was interposed beyond the contact-wires, both the power of affecting the multiplier, and of giving polarity to the needle in the spiral, was arrested; and this was uniformly the result in a considerable number of experiments made with three different fishes, of which two were very active, and with perfect conductors, free of this interruption, produced both effects readily. But the power of giving a shock was not equally arrested; for on removing the multiplier and spirals, and holding with the wet fingers the wires attached to the moist cotton thread, the shock was several times distinctly felt on stimulating the fish. The space of cotton thread between the wires was about one tenth of an inch, and to secure its perfect humidity or wetness, it was inclosed in a glass tube, with corks at each end, through which the wires passed.

When the apparatus already described in noticing the chemical effects of the torpedo, was substituted for the wet cotton thread, the tubes being filled with a strong solution of salt, the multiplier was affected, and gas was given off at each of the points of the gold wires, and when steel needles were used, a fine current of gas rose from the point connected with the under contact-wire, and not a particle from the other point. In these experiments, there were interposed, at the same time, the chemical apparatus, one on each side, the spiral, one also on each side, and the multiplier intermediate, and there were necessarily many junctions of wires. I scarcely need add, that in an experiment made expressly to ascertain it, the shock of the fish was felt beyond the saline solution; for it had been previously proved, by the experiments of Mr. Walsh, that salt water, even in a long circuit of imperfect conductors, has the power of transmitting it.

2. *Observations on the Electrical Organs of the Torpedo, and on some parts of its structure connected with them.*

The peculiar columnar appearance of the electrical organs of the torpedo, their great proportional size, the vast proportion of nerve with which they are supplied, the manner in which the columns are sheathed in tendinous fibres, have been dwelt on by all inquirers who have paid any attention to this fish; but I am not acquainted with any attempt to ascertain, by experiment, what is the exact nature of the substance of these organs, or the peculiar structure of which they are composed.

When I have examined, with a single lens which magnifies more than two hundred times, a column of the electrical organs, it has not exhibited any regular structure; it has appeared as a homogeneous mass, with a few fibres passing into it in irregular directions, which were probably nervous fibres.

The specific gravity of the electrical organ, in comparison with that of parts of the fish decidedly muscular, is very low; including the upper and under boundary of skin, I have found it 1·026, to water as 1·000. The specific gravity of a portion of the abdominal muscles of the same full-grown fish, was 1·058, and that of the thick strong muscles of the back close to the spine 1·065.

The loss of weight which the electrical organ sustains by drying, is greater than I have observed in any other part of the fish. I shall give the results of one trial; the statement will convey an idea of the bulk of the different parts of the torpedo, as well as of the proportion of solid matter which they contain. The subject of the experiment, procured fresh from the fish-market at Rome, was eight inches long, and across the widest part five inches broad. Entire it weighed 2065 grains. It was carefully divided, and the different parts mentioned were found to weigh as follows, in their moist state:

	Grains.
Spleen	5·5
Pancreas	5·0
Testes	3·0
Kidneys	8·0
A pale cream-coloured oval body close to left kidney	0·25
A reddish oval body, like a gland, attached to the large intestine	0·5
Liver, with gall-bladder and ducts	105·0
Heart, and trunk of pulmonary artery	3·0
Gills, including branchial cartilages	53·0
Gullet	11·0
Stomach	65·0

	Grains.
Upper valvular intestine	29·0
Lower intestine	5·0
Electrical organs	302·0
Head, separated at first vertebra	165·0
Thorax, consisting of cartilaginous case and muscles, with pectoral fins attached	670·0
Abdomen, without its contents	440·0
Tail, separated just below the anus	195·0

By exposure to the heat of boiling water for about sixteen hours, the different parts were completely dried; their total weight was reduced to 322 grains, so that they had lost by drying 84·5 per cent.

	Grains.
The electrical organs now weighed	22
Head	25
Thorax	93
Abdomen	53
Tail	36
Liver (abounding in oil)	43
Residue, consisting of other organs and extract of fluids, which exuded during the drying	50

From the above loss of weight of the electrical organs in drying, they appear to consist of 7·28 matter *not evaporable* at 212° FAHR. and of 92·72 water, taking it for granted that the loss sustained is owing merely to the evaporation of the aqueous part. I lay stress on matter not evaporable, because I believe that the solid contents of the moist organs are less, and that the water which they contain holds in solution various substances.

This solution may be obtained by cutting the electrical organs into small pieces, and placing them in a funnel; the fluid part slowly separates. What I have thus collected was slightly turbid, of a very light fawn colour, just perceptibly acrid; it did not change the colour of turmeric or litmus paper; a cloudiness was occasioned by dropping into it a solution of nitrate of silver, which was not completely re-dissolved by aqua ammoniæ; it was copiously precipitated by acetate of lead, and a cloudiness was occasioned in it by nitrate of barytes and by corrosive sublimate. By evaporation, it afforded a residue which deliquesced partially on exposure to a moist atmosphere, and had an acrid and bitter saline taste. The exact proportion of this weak solution of animal and saline matters, I have not ascertained; and, indeed, it would be very difficult to determine it with any degree of accuracy, for only a small portion separates spontaneously, and if pressure be used, the fibres are broken, and the expressed fluid is mixed with a pulpy matter.

When the electrical organs of the torpedo are immersed in boiling water, they suddenly contract in all their dimensions, and the columns, from pentagonal, which they generally are, become circular. In my early experiments at Rome, they were rendered firmer by immersion for a few minutes, and the columns appeared to be tolerably distinctly fibrous and laminated, bringing to recollection the structure of the pile of ZAMBONI. Latterly I have not witnessed this effect; in a few seconds the tendinous fibres have been converted into jelly, and the columns have fallen asunder, having the appearance and consistence of a translucent, very soft mucilage. To what this difference of effect may be owing, I am at a loss to conceive; perhaps the Roman fish were older than the Maltese, or the aqueduct water at Rome may be harder than the rain cistern water of Malta.

On exposure to the air in a damp atmosphere, or by maceration in water, changing the water daily, the electrical organs undergo change more slowly than the parts distinctly muscular; in putrefaction and maceration they have less resemblance to muscular fibre than to tendinous fibre, which latter offers great resistance to both these processes. But I would not lay any stress on this quality of resistance, as it is vague, depending on circumstances which it is extremely difficult to appreciate, as every one must be convinced who has compared the different degrees of rapidity with which different orders of muscles in man and the larger mammalia undergo change from putrefaction and maceration; for instance, the slowness with which the muscular fibres of the stomach and intestines alter, and the rapidity of change of the fibres of the heart and thick muscles.

Quitting the organs of the dead fish, I shall now notice the few observations which I have made on them, before they have been deprived of their vitality.

The effect of the electricity of a small voltaic trough, the shock of which I could just perceive at the extremities of the moistened fingers, was very distinct on the voluntary muscles of a live torpedo just taken from the water; but it did not appear to affect in the least the electrical organs. I could not perceive the slightest contraction of them in whatever manner the wires were applied, not even when a minute portion of integument was removed, or when one of the wires was placed in contact with a fasciculus of the electrical nerves. Even after apparent death many of the parts decidedly muscular continued to contract under this stimulus, especially the muscles of the flank and the cross muscles of the inferior surface of the thorax and the heart; indeed this latter organ, two hours after it had been removed from the body, and had ceased to contract spontaneously, renewed its contractions under the galvanic influence. Other stimulants have been applied to the electrical organs, and with the same negative result. Even when punctured and incised, (a portion of their skin having been removed, which appears to be very sensitive,) no indications whatever were witnessed of their substance being either sensitive or contractile.

Reflecting on the facts and observations which I have just detailed, it appears to me very difficult to resist the conclusion, that the electrical organs of the torpedo are not muscular, but columns formed of tendinous and nervous fibres distended by a thin gelatinous fluid. Their situation too, surrounded by and exposed to the pressure of powerful muscles, shows that if condensation is required for the exercise of the electrical function, they may experience it without possessing any muscular fibres in their own substance. The arrangement of the muscles of the back and of the fins, and of the very powerful cross muscles situated between the under surfaces of the electrical organs, is admirably adapted to compress them. Without entering into any minute anatomical examination of these muscles and their uses, it is only necessary to compare them in the torpedo and in any other species of Ray, to be convinced that they are adequate to and designed for the effect mentioned.

Mr. Hunter, in his account of the torpedo *, describes the columns of the electrical organs as composed of cells containing a fluid, divided by their horizontal partitions, which he was able to count. This structure seems very probable, and in the specimens I dissected at Rome, I saw what I fancied an

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* Phil. Trans. 1773.

approach to it; but I have never witnessed it in a satisfactory manner in the fresh fish. Mr. HUNTER inspected large fishes which had been preserved in spirits. The partitions of the columns in them might have been more visible, (supposing them to exist,) from the action of the spirit on the membrane, and from the greater size of the specimen; or they might have been formed after death, in the spirits, by a slow deposition of the animal matter contained in the columns.

Next to the nature of the substance of the electrical organs, the electrical nerves have occupied my attention. Their three great trunks have been accurately described by Mr. HUNTER; but this distinguished anatomist has very briefly noticed their distribution, which is curious, and deserving, I believe, of minuter investigation. I shall attempt little more than an outline of what I have observed in some dissections conducted with considerable care.

In examining the brain, proceeding from the anterior to the posterior portion, after passing the first, second, third, and fourth pair of nerves, or the olfactory, optic, motor and pathetic nerves of the eye, the fifth pair is seen issuing from the medulla oblongata, or posterior tubercle of the brain *. After quitting the cranium, (confining the description to one side,) it proceeds upwards, divides into two large branches, which go to clusters of mucous glands situated in the front of the head and at the anterior margin of the electrical organs, and they appear to be confined to these parts. The next pair, the first electrical, rises close to the preceding, just behind it, and in passing out of the cranium is firmly connected with it; and also where it passes out, a portion of medullary matter proceeds from it into a cavity filled with fluid, in the cartilage adjoining, which there is reason to consider as the cavity of the organ of hearing, and the medullary matter the nerve of hearing. After this, in passing outwards, it divides into three small branches and two large ones. Of the former, one proceeds to the gills, another to the adjoining muscles, and the third to the mouth. Of the great branches, one ascends, and sweeping round the margin of the electrical organ is distributed to the mucous glands which abound there, and where some of its twigs inosculate with twigs of the former nerve. The other great branch, which is inferior, enters the electrical

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* The nerves of the fourth pair are so very small and tender, that it is difficult to demonstrate them, excepting in old and large torpedos.

organ and ramifies through its superior portion. The next pair of nerves, the second electrical, rises a little beyond the preceding. On leaving the cranium it divides into two great branches; these, with the exception of nervous twigs supplying the adjoining branchiæ, are distributed entirely in the substance of the electrical organ and ramify in all directions through its middle portion. The third electrical rises close to the last, divided only by a very thin plate of cartilage; the principal portion of it passes into the electrical organ and ramifies through its inferior part, and besides, gives off three small branches, which are sent to the adjoining branchiæ, to the gullet and stomach, and to the tail. The branch which supplies the stomach appears to be the principal nerve of this organ; it descends along the inner and inferior portion of the gullet, and ramifies in the direction of the great arch of the stomach. The caudal branch descends in a straight line under the peritoneal lining of the abdomen, and under the spinal nerves, without giving off a single branch till it reaches the tail, in the muscular substance of which it is lost.

I have not yet been able to discover any connexions of the electrical nerves, besides those pointed out. It is an interesting fact that the gastric nerves are derived from them. Perhaps superfluous electricity, when not required for the defence of the animal, may be directed to this organ to promote digestion. In the instance of a fish which I had in my possession alive many days, and which was frequently excited to give shocks, digestion appeared to have been completely arrested; when it died, a small fish was found in its stomach, much in the same state as when it was swallowed;—no portion of it had been dissolved.

Though I have not found the temperature of the electrical organs higher than that of other parts of the fish, or the temperature of the fish generally different from that of the water in which it has been confined, yet it seems probable that as the branchiæ are liberally supplied with twigs of the electrical nerves, there may be some connexion between its respiratory and electrical function; and I venture to offer the conjecture, that by means of its electricity it may have the power of decomposing water and of supplying itself with air, when lying covered with mud or sand in situations in which it is easy to conceive pure air may be deficient; and, in my experiments, I have often fancied that I have witnessed something of the kind,—after repeated discharges of its electricity, the margin of the pectoral fins has acquired an appearance as if very minute bubbles of air were generated in it and confined.

Besides the electrical nerves there is a plexus of nerves deserving attention, of great magnitude, formed by the junction of the anterior and posterior, or upper and under cervical nerves; of the former about seventeen on each side, of the latter about fourteen [*]. It makes its appearance as one trunk just below the transverse cartilage which is interposed between the thorax and abdomen. It sends a recurrent branch to the muscles and skin of the under surface of the thorax; but its main trunk ascends along the inner margin of the pectoral fin, and is distributed through it. On this plexus the sentient and motive powers of the parts connected with the electrical organs seem to depend.

The electrical nerves at their origin are enveloped in a very thick fibrous sheath. As the branches subdivide in the substance of the organ, the neurilema becomes thin and semitransparent. On examining a minute branch with a powerful lens, its internal or medullary substance is not seen in a continuous line, but interrupted, as it were dotted, as if the sheath contained a succession of portions with a little space between each.

In the anatomical structure of the torpedo, the mucous system forms a very conspicuous part; it consists of several clusters and chains of glands distributed chiefly around the electrical organs, at different depths beneath the cutis; and of strong transparent vessels, of various lengths and sizes, opening externally in the skin, for the purpose of pouring out the thick mucus secreted by the glands, and destined for lubricating the surface. This system has not been noticed by Mr. HUNTER, and it has been but imperfectly described by LORENZINI [†]. Though it is not peculiar to the torpedo, it is much more strongly developed in this fish than in any other species of Ray with which I am acquainted, and the situation of the glands and the distribution of their vessels are different. Whether it is concerned in any way with the electrical function of the torpedo is deserving of consideration. That it is thus concerned in some way, seems to be indicated, not only by the situation of these glands, between and surrounding the electrical organs, but still more so by the manner in which they are supplied with nerves, either from the first electrical, or from the fourth pair, which is connected with that nerve. As the thick semitransparent mucus which these glands secrete, is probably a better conductor of electricity than the skin alone, or than salt water, this mucous system may serve as a medium of communication between the electrical organs *. I shall mention some results which are favourable to this idea. When one contact-wire was placed underneath an active torpedo, just anterior to the mouth, and the other at the extremity of the back, out of the circle of the mucous apparatus, the shock of the fish had no effect either on the multiplier, or on needles in the spiral. But when the upper contact-wire was made to touch the back of one electrical organ, the under wire being placed as in the preceding experiment, then both effects were simultaneously produced; and they were also produced when the two wires were brought very close to each other, one being kept as before, and the other moved immediately over it, in front, each about a quarter of an inch from the margin, and not connected with the electrical organs, except by the common integuments and this mucous apparatus. It is worthy of remark, that this little space in front, intermediate between the two electrical organs, so abounding in glandular structure, and so amply provided with nerves, appears from experiment to possess very little sensibility; this was denoted in these trials, in which the fish, though exquisitely sensible of pressure on the margin of the pectoral fins, seemed indifferent to it when applied in front,—as if the fourth pair, which supplies this part, were destined rather for secretion than for the purpose of sensation.

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[*] Towards the origin of the spinal cord there is a small space, from the under surface of which six nerves arise, three on each side; but none from the upper surface, whence the difference of number noticed in the text.

[†] Osservazioni intorno alle Torpedini fatte da STEFFANO LORENZINI Fiorentino; 4to, Firenze, 1678.

The connexion between the electrical nerves and the mucous system, even more remarkable than between the former and the stomach, may perhaps warrant the conjecture, that the electrical function may not only be aided by, but also aid the secretion of mucus; and that, as was supposed in regard to the stomach, when the electricity is not employed in repelling an enemy in violent efforts, it may be exercised gently in increasing the activity of these glands.

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* Some comparative experiments which I have made seem to indicate that the mucus of the torpedo is a better conductor than sea water; when the hands were smeared with this mucus, or when a portion of the fresh skin of a torpedo, with its natural mucus adhering to it, was wrapped round the ends of the contact-wires by which they were held, the shock received appeared to be stronger than usual.

In support of this notion it may be mentioned, that in the fishes which I have kept, in which digestion was arrested, the secretion also of mucus appeared to be stopped or considerably diminished.

Mr. HUNTER, from the examination of a torpedo whose vascular system was injected, states that the electrical organs of this fish are abundantly supplied with blood-vessels. From what I have witnessed in the living fish and the fresh fish recently dead, I am compelled to conclude that the quantity of blood which circulates through them is very inconsiderable. The blood-vessels which pass into them with the electrical nerves are small; the organs are colourless, and very few branches carrying red blood are perceptible extending through them. The integuments of these organs, and the pectoral fins, and lateral clusters of mucous glands are indeed abundantly supplied with blood-vessels. The contrast of the vascularity of these parts and of the electrical organs, is so strongly marked as to suggest the idea that the latter can possess very little ordinary vital activity, and that in accordance with the common analogies of living parts they must be rather passive than active.

3. *Concluding Remarks.*

The experiments which I have detailed on the electricity of the torpedo confirm those of Mr. WALSH made in 1772, showing its resemblance to common electricity. They moreover show, that, like common electricity and voltaic electricity, it has the power of giving magnetic polarity to iron, and of producing certain chemical changes. In these its general effects it does not seem to be essentially peculiar, but as much allied to voltaic electricity as voltaic electricity is to atmospheric, or atmospheric electricity is to that produced by contact or friction.

When we examine more minutely its phenomena or effects, in relation to these different kinds, or varieties of electricity, certain points of difference occur.

Compared with voltaic electricity, its effect on the multiplier is feeble; its power of decomposing water and metallic solutions is inconsiderable; but its power of giving a shock is great, and so also is its power of magnetising iron.

Compared with common electricity, it has a power of affecting the multiplier, which under ordinary circumstances common electricity does not exhibit; its chemical effects are more distinct; its power of magnetising iron, and giving a shock appear very similar*; its power of passing through air is infinitely less, as is also (if it possess it at all) its power of producing heat and light.

There are other points of difference; I allude chiefly to the results obtained in the experiments already described, in which the metallic communication was interrupted by a strong solution of salt. In this instance the full power of the fish appeared to pass; water was decomposed, a shock was received, needles were magnetised, and the multiplier was affected. When the same experiment was made on the electricity excited by the small voltaic combination of a single plate of copper and zinc, each less than an inch in length, and half an inch in breadth, immersed in an acid, neither water was decomposed nor was the multiplier affected. When it was made on the electricity of the electrical machine by means of a Leyden jar, all the effects were witnessed excepting the motion of the multiplier, and the order of succession of poles in the needles magnetised in the spirals.

How are these differences to be explained? Do they admit of explanation similar to that advanced by Mr. CAVENDISH in his theory of the torpedo; or may we suppose, according to the analogy of the solar ray, that the electrical power, whether excited by the common machine, or by the voltaic battery, or by the torpedo, is not a simple power, but a combination of powers, which may occur variously associated, and produce all the varieties of electricity with which we are acquainted?

As regards the mode of production, or the cause of the electricity of the torpedo, it is unavoidably enveloped in great mystery. Like animal heat, and the light emitted by certain animals, and, I may add, like the secretions of animals generally, it appears to be a result of living action, and connected with a peculiar and unusually complicated organization. All the attempts I have made to obtain electrical excitement in the fish, after it has been deprived of life, have been in vain.

The observations which I have detailed relating to its anatomical structure show a complicated adaptation of parts, nerves of unusual magnitude ramifying between apparently insensible columns, saturated with a bad conducting fluid; muscles surrounding these columns and fitted to compress them; and a system of mucous glands and tubes adjoining, well adapted to be the medium of electrical communication between the two organs and their opposite sides.

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* There is this difference when two spirals are used, one connected with the inside of a Leyden jar, and the other with the outside,—a needle in each similarly placed acquires opposite polarities, the north pole in one being where the south pole is in the other; whilst in the instance of the torpedo they accord, so that a line of needles passing from one side of the electrical organ to the other would exhibit a succession of similar poles.

When we consider this structure, it is an easy matter to trace rude analogies between it and the pile of Volta,—or between its columns and a battery of Leyden jars, such a battery as was formed by Mr. Cavendish for imitating the electricity of the torpedo, composed of a large number of jars of very thin glass, feebly charged. But these analogies seem to help very little, if at all, towards the solution of the great difficulty; the question remains unanswered, What is the cause or source of the electricity? Here analogy fails entirely; none of the ordinary modes of excitement appear to be at all concerned; neither friction, nor chemical action, nor change of temperature, nor change of form. Let us consider for a moment a small torpedo in an active state. The smallest which I have employed in my experiments weighed only 410 grains, and contained only 48 grains of solid matter; its electrical organs weighed only 150 grains, and contained only 14 grains of solid matter,—for to this they were reduced by thorough drying. Yet this small mass of matter gave sharp shocks, converted needles into magnets, affected distinctly the multiplier, and acted as a chemical agent, effecting the decomposition of water, &c. A priori, how inconceivable that these effects could be so produced! This fish was about ten days in my possession, during the whole of which time it ate nothing, and its bulk was hardly sensibly altered; and every day it exercised its electrical powers, and to the last they appeared almost as energetic as when it was fresh from the sea. This adds, if possible, to the difficulty of explanation. That this mysterious function is intimately connected with the nerves, and in a manner more striking than all ordinary secretions, is manifest. Beyond this conclusion all is darkness; we have not, as we have in the doctrine of animal heat, advanced another step; we have not been able to connect it with changes in the electrical organs as analogous to known sources of electricity, as the changes which take place in the lungs in respiration are to the known sources of heat or combustion. The attainment of this step is a great desideratum; and beyond it, probably, we shall never be able to proceed.

Without reverting to the conjectures which, in passing, I have offered on the subserviency of the electricity of the torpedo in an auxiliary manner to digestion, respiration and the secretion of mucus, I may remark that its chief use appears to be for purposes of defence, to guard it from its enemies, rather than to enable it, according to vulgar opinion, to destroy its prey and provide itself with food. Small smelts, which I kept in the same vessel with torpedos, appeared to have no dread of them, and I believe they fed on their mucus; and, in an experiment in which, in a confined space, I excited an active torpedo to give shocks, a smelt which was with it was evidently alarmed, and once or twice, when exposed to the shock, leapt nearly out of the vessel; but was not injured by the electricity. In confirmation I may add, that the electric power of the young fish, which most requires it for its protection, is proportionally very much greater than that of the old, and can be exerted without exhaustion and loss of life much more frequently. After a very few shocks most of the old fish which I have had, have become languid, and have died in a few hours, whilst young ones from three to six inches long have remained active during ten or fifteen days, and have never failed to show the effects I have described.

Before concluding, I could wish to explain the difference of the results of the experiments made by my brother, and of those I have detailed; but I must confess my inability to do it in a satisfactory manner. Knowing his great accuracy in experimenting, I am confident that their failure, or negative results must have depended on some circumstance deserving of investigation, and which I hoped by inquiry to discover.

I once imagined that they might have depended on the kind, or variety of fish employed. But the experiments I have made with a view to this have not borne me out in the conjecture. I have tried very many different specimens of the two varieties of the torpedo most common in the Mediterranean, the mottled and the spotted, called at Rome Tremola and Occhiatella, without perceiving any distinguishable difference of electrical effect.

It appeared possible that the sex of the fish might have some influence on its electricity, or that in the instance of the female fish, the state of the ovaries whether pregnant or not, might have an influence. But observation does not confirm the probability of either opinion. I have used, I believe, as many males as females in my experiments, and the results with both have been very similar. Though the great breeding season appears to be in spring, females containing eggs variously advanced are to be met with occasionally both in summer and autumn, and comparing their electricity with that of barren or unimpregnated fish, I cannot say I can be sure of any well marked difference; if there were any difference, the electricity of the former was most powerful.

I have sometimes imagined that the age of the torpedo might modify its electrical effects, and that the older the fish is, the more analogous it is to the Leyden jar, and the younger it is, the more analogous it is to the voltaic battery. Many comparative trials of fishes of different ages appeared to favour this notion. But I soon had an opportunity of ascertaining that it is not universally true; the largest torpedo I have yet obtained disproved it. This fish, a female Tremola, was sixteen inches and a half long, and seven inches and a half broad, in a languid state, having been caught several hours and kept in a small quantity of water; yet a single discharge of its electricity produced a complete revolution of the needle in the multiplier, magnetised feebly four bars of steel weighing seventy-five grains, and magnetised powerfully two small sewing-needles; one of which acquired the power of supporting three times its weight of iron. Nor were the chemical effects produced by this fish less distinct.

Besides the preceding, other probable causes of the difference of results I could wish to explain might be pointed out; but as I have not had an opportunity of submitting them to the proof of experiment, it would be trespassing on the time of the Society to bring them forward.

Malta, September 30th, 1831.

XIII. *Experimental Researches in Voltaic Electricity and Electro-Magnetism.*

By the Rev. William Ritchie, *LL.D. F.R.S. Professor of Natural and Experimental Philosophy in the Royal Institution of Great Britain, and Professor of Natural Philosophy and Astronomy in the University of London.*

Read January 19, 1832.

THE splendid discoveries which have lately been made in magnetism and electro-magnetism have so much engaged the attention of philosophers, that the theory and laws of action of voltaic electricity, no longer possessing the charms of novelty, have been entirely neglected. The subject appearing to me full of interest, and lying at the very foundation of a large portion of physical science, induced me to undertake an experimental investigation of some of the most important points connected with it, the result of which I have the honour of laying before the Royal Society.

PART I.

ON THE LAWS OF ACTION OF AN ELEMENTARY BATTERY.

1. Volta was led to the invention of the pile by what he conceived to be the discovery of a new power in nature, viz. the development of electricity by the simple contact of dissimilar metals. Other philosophers have denied the existence of this power, and have substituted that of chemical action in its stead; whilst a third class still maintain that both powers are concerned in the production of voltaic effects. We have lately had a series of experiments by M. Matteucci to prove that motions could be excited in the limbs of a frog by carefully washing it in distilled water, and then acting on it with discs of copper and zinc. These experiments were intended to prove that galvanic action resulted from the simple contact of dissimilar metals, without the aid of chemical action, and that consequently the theory of Volta was well founded.

It is easy to neutralize the effects of these experiments by one much more striking, in which decided voltaic effects are produced by one metal and one liquid.

The best mode of proving this is by the following experiment:

Exp. I.—Form a galvanometer with a coil of copper wire, and leave the ends projecting about two feet. Roll one of the ends about a small rod so as to form a close spiral about a quarter of an inch in diameter. Roll the other end of the wire about a large rod or glass tube so as to form another spiral of half an inch in diameter. Place the small spiral within the larger one, and immerse them in water containing a quantity of nitric acid, and a very considerable electro-magnetic effect will be produced.

I have given this experiment to prove, in a manner free from every objection, that voltaic action may be produced without the contact of dissimilar metals, and consequently without the aid of that mysterious force, termed by Volta and his followers *electro-motive*.

2. Those who adopt the theory of Volta have taken it for granted, without a shadow of proof, that the free positive electricity which they conceived they had detected on the surface of the zinc, was that which circulated through the liquid and metallic conductors, and produced all the phenomena of voltaic electricity. M. Parrot of St. Petersburgh has examined the fundamental experiments of Volta with the most scrupulous regard to accuracy, and observed that sometimes a minute portion of free positive electricity was detected on the zinc and at other times on the copper. This minute portion was obviously developed by friction or simple pressure of the zinc and copper plates; for when the plates were soldered together, and the experiment repeated, as described by Volta, he could not detect the slightest sign of free electricity*. From the marked difference between the effects of free and voltaic electricity, it is extremely improbable that a minute portion of common electricity could ever acquire the characters of voltaic. Common electricity is diffused over the surface of the metal;—voltaic electricity exists within the metal. Free electricity is conducted over the surface of the thinnest gold-leaf, as effectually as over a mass of metal having the same surface;—voltaic electricity requires thickness of metal for its conduction.

3. A powerful argument against the theory of free electricity becoming com-

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* Annales de Chimie, xlvi. 363.

bined or voltaic when connected plates are immersed in a conducting liquid, is derived from the experiments of M. BECQUEREL, combined with the following. M. BECQUEREL found that if one end of a metallic wire be heated by a spirit lamp, it becomes positive, whilst the cold end is negative. If a platina wire be placed on the cap of a gold-leaf electrometer, and the projecting end heated and then touched with a piece of heated glass or moistened paper so as to remove the positive electricity, the gold-leaves will immediately diverge by negative electricity. It becomes then an important question to ascertain if the free electricity thus developed, changes its character and becomes voltaic. This was accomplished by the next experiment.

EXP. II. Having connected two slips of platina by copper wires with the cups of a galvanometer, I heated the end of one of the pieces, and immersed both, parallel to one another, in diluted nitric acid, when only a slight effect was produced on the needle. I then substituted iron for platina, and repeated the experiment, when a powerful effect was produced. With copper, the effect was somewhat less. With zinc, the effect was considerable: but with antimony and bismuth scarcely any effect could be observed. But what is most remarkable is the fact, that in all the cases the cold metal is positive and the hot negative; or in other words, the cold metal has the same relation to the hot, that zinc has to copper in an ordinary voltaic arrangement. This experiment demonstrates that the free electricity developed by heat has no connexion with that developed by voltaic action: since the effects of heat in developing free electricity in platina is much greater than in iron; whereas the voltaic electricity developed in iron is much greater than that developed in platina, and both of an opposite character. Since this portion of free electricity developed by heat does not become voltaic, it is exceedingly improbable that the electricity developed by the contact or pressure of metals should by immersion in a liquid acquire this character.

4. In both theories of voltaic electricity it is admitted that the zinc is positive, and the copper negative. The analogy between common and voltaic electricity seems to me to have been pushed too far. I have carefully sought for the proof of this principle, but have been unable to find any. We have already shown that the experiments of M. PARROT are quite conclusive against the truth of the experiments of VOLTA. Again: in the dry pile of DE LUC, free posi-

tive electricity is developed at the zinc end, and negative at the copper end. But the electricity developed in the dry pile has not one character in common with voltaic. It makes gold-leaves diverge;—voltaic does not. It is most energetic with an imperfect conductor between the plates;—voltaic on the contrary increases with the conducting power of the fluid interposed. This electricity will not decompose water;—a slight development of voltaic does so, energetically. This pile is only in action when the poles are not connected;—voltaic action does not exist unless the poles be connected. The experiment of Dr. Wollaston in which he decomposed water by common electricity might seem at variance with this reasoning. But this decomposition is totally unlike that produced by voltaic electricity; for, as Dr. Wollaston remarks, a mixture of oxygen and hydrogen rose from each of the fine metallic points, a fact which shows that the decomposition was produced in a manner essentially different. The decomposition in this experiment seems to have been effected by the mechanical agency of the electric fluid. The fine electric dart shooting out from the invisible gold points may have actually cleaved a molecule of water which happened to be favourably situated, and thus its oxygen and hydrogen were disengaged at the point where the mechanical cleavage took place *.

5. It does not appear to me at all necessary that zinc and copper should be thrown into opposite electric states to produce voltaic action. I shall make no suppositions with regard to those states, but ground my views of voltaic action on well established facts. Zinc has a much more powerful attraction for oxygen than copper; and yet copper has also a decided attraction for it, otherwise there could be no salts of this metal. Let us now suppose, merely for the sake of illustration, that a molecule of water is composed of an atom of oxygen united to an atom of hydrogen. In the Plate VIII. (fig. 1.) let the oxygen be represented by the white circle, and the hydrogen by the black. The zinc plate *z*, having a greater attraction for the oxygen than for the hydrogen, will turn round the molecule of water in contact with it, till its oxygen side be towards the zinc, and its opposite side towards the copper plate *c*, which is connected with the zinc by the wire *w*. The same thing will take place with

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* The same remarks apply to Mr. Barry's experiments on decomposition by atmospheric electricity. Decomposition was never effected by common electricity in which the component parts of the substance were liberated at opposite poles.

Fig. 1.

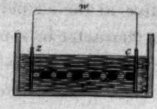


Fig. 2.

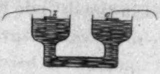


Fig 3.

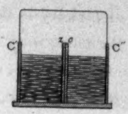


Fig. 4.

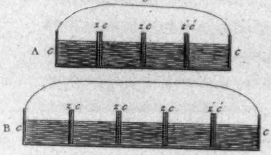


Fig. 5.

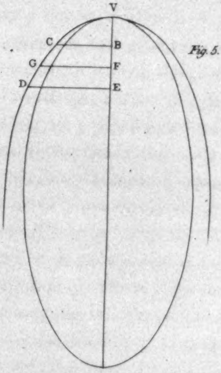


Fig. 6.

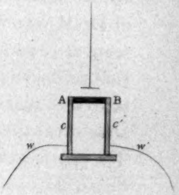


Fig. 8.

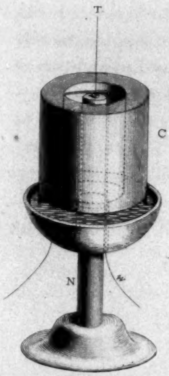


Fig. 7.

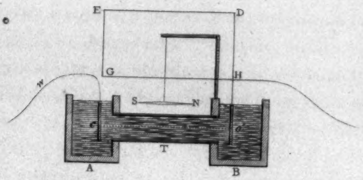


Fig. 9.

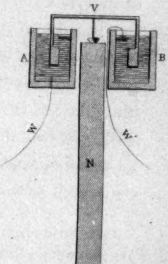
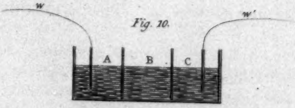


Fig. 10.



J. Basire sc.

the other molecules, till the whole chain of aqueous particles be arranged in this definite order. The component parts of the electric fluid naturally belonging to the oxygen and hydrogen will also assume a definite arrangement. The component parts of the electric fluid, thus arranged, will act by induction on the neutral electricity belonging to the metallic plates and connecting wire, and thus produce a definite arrangement of the molecules of the electric fluid along the whole metallic circuit *.

If the attraction of the oxygen for the hydrogen be stronger than the attraction of one of the metals for the oxygen, the water cannot be decomposed; and yet this definite arrangement may take place, and consequently there may be decided electro-magnetic effects without chemical decomposition. Hence diluted alcohol when placed between the copper and zinc plates, develops voltaic electricity without the slightest trace of decomposition.

If the attraction of the zinc for the oxygen be greater than that of the oxygen for the hydrogen, the oxygen will combine with the zinc, and the hydrogen will be set at liberty. This hydrogen must therefore either be transported through the liquid to the copper, or attach itself to the oxygen of the next molecule of water, and set its hydrogen at liberty; and so on, till the last atom of hydrogen in contact with the copper plate, having nothing to combine with, escapes in its gaseous state. It is difficult to conceive how hydrogen could be dragged through the intervening mass of liquid with equal facility in every direction, and even when the plates are separated by a moistened diaphragm of bladder. The view we have taken of voltaic action, without any actual transfer of hydrogen, appears the most simple and natural; and striking illustrations of its truth will be given in future experiments.

When an atom of oxygen is separated from the hydrogen at the surface of

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* Since this paper was written, these views have received a striking confirmation from the splendid discoveries of Mr. FARADAY. That ingenious philosopher has proved, by the clearest evidence, that the neutral electric fluid, essentially belonging to a metallic wire, may be *decomposed* by the inductive power of a common magnet, and has even obtained an electric spark from a temporary magnet, *the only magnet from which a spark has been obtained*; for, in the experiments of NOBILI, in which a common magnet is used, it is still from a *temporary* magnet that the spark is ultimately obtained. To render the analogy more striking, I have succeeded in exploding a mixture of oxygen and hydrogen gases by the spark obtained from the induction of a common magnet, without any actual transfer of electricity from the magnet to the conductor.

the zinc plate, the molecules of water must turn round their axes till the definite arrangement of the particles again take place. This revolution of the particles of water must obviously produce, by induction, a similar revolution of the molecules of the electric fluid round the distinct elementary particles of which the metallic conductor is composed, agreeably to the ingenious theory of M. Ampère.

From this view of the subject, it is obvious that whatever will render the water more easily decomposed will also increase the power of the voltaic arrangement. If the temperature of water be raised, the attraction between its molecules will be diminished; it will therefore become more fluid, its molecules will be turned round with a smaller force, and arrange themselves in the definite order which seems essential to voltaic action. Again: strong sulphuric acid is an imperfect conductor; pure water is also a bad conductor; but if they be mixed together, we get a liquid of high conducting powers. Now, according to the theory of an actual transfer of electric fluid, this is exceedingly mysterious; but is a necessary consequence of the view now given. When water is mixed with sulphuric acid, the attraction between its own molecules must be diminished, and consequently acidulated water will be more easily decomposed than pure water, and will consequently produce more powerful effects when placed between the copper and zinc plates in a voltaic arrangement.

6. If this view of the subject be correct, it follows that all liquids whose component parts go to the same pole, are non-conductors of voltaic electricity. Oils, resinous substances, melted camphor, caoutchouc, &c. are hence non-conductors. The liquified gases, examined by Mr. Kemp, submit to the same law. Liquified sulphurous acid is a good conductor, because oxygen and sulphur, its component parts, go to opposite poles. Liquified ammoniacal gas is doubtful as to its conducting power. Hydrogen goes decidedly to the negative pole, but nitrogen seems doubtful to which pole it belongs; and hence Mr. Kemp, without any view of supporting a favourite theory, could not determine with certainty whether this substance was a conductor, or not. It obviously follows from this view of conduction, that all simple substances (except the metals,) in a fluid state are essentially non-conductors. When liquified chlorine was submitted to the same test, it was found to be a perfect non-conductor. This affords another beautiful illustration of the simple nature of chlorine.

7. In examining the conducting power of alcohol when placed between platina discs connected with a powerful battery, I was at first surprised to find a gas given off at the negative pole, without the slightest appearance of anything being separated at the positive pole. After collecting a small portion of the gas, I found it to be pure olefiant gas.

Now, alcohol being composed of oxygen, hydrogen and carbon, in the proportions which constitute water and olefiant gas, it is obvious that water, without suffering decomposition, must have been separated at the positive pole. This is indeed what might have been expected. Water, being composed of oxygen and hydrogen, must have a greater tendency to the positive pole than olefiant gas, the component parts of which have both a decided tendency to the negative pole. When the alcohol is diluted, it becomes a better conductor, in consequence of its becoming more easily decomposed. The water with which it has been diluted does not suffer decomposition, but performs the same office with regard to alcohol that sulphuric does when mixed with water.

8. It obviously follows from this view of conduction, that a liquid has a very confined limit to its conducting power, or, in other words, that a section of a liquid will only conduct a given quantity of electric influence. This was established by the following experiment.

Exp. III. Having drawn out a glass tube in the middle, by means of a blowpipe, and bent it into the shape of the letter U, as in fig. 2, I filled it with diluted acid.

Discs of copper and zinc of the same diameter with the narrow part of the tube, and connected with the torsion galvanometer, were immersed at *z* and *c*, and the deflecting force ascertained. Having removed these plates, and substituted others several times larger, very little increase of effect was observed. It appears from this experiment, that the water in the narrow part of the tube had been arranged in the definite order, and that an increase of metallic surface had very little effect in modifying the arrangement.

9. If this view of conduction be correct, there can be no actual transfer of electricity along those substances which are called conductors, as in the case of common electricity; the whole of the effects depending on the definite arrangement of the molecules of the electric fluid, essentially belonging to the conducting substance. Let us suppose, for the sake of illustration, that the

atoms of the vitreous and resinous elements possess polarity, or have the strongest tendency to unite at opposite points. These poles will obviously be arranged facing each other in a copper and zinc plate, forming an elementary battery. This arrangement will re-act on the chain of aqueous molecules, till the maximum of effect take place. The electric fluid thus arranged in the zinc and copper plates will be partially retained by the coercitive force of the metal. Hence it follows that the electric molecules might be so arranged as to have opposite poles turned to opposite sides of a thin plate of metal; or, to use the usual mode of expression, a thin plate of metal might have one side rendered positive and the other negative. The best mode of exhibiting this property, which seems first to have been observed by Ritter, is the following.

Exp. IV. Cement three very thin copper plates, about two inches square, in a wooden trough, at the distance of half an inch from each other, having copper wires soldered to each. Connect the extreme plates with the ends of a powerful battery, the spaces between them being previously filled with diluted acid, and allow decomposition to go on for a few minutes. Remove the battery, and connect one of the extreme plates and the middle one with a galvanometer, and very decided electro-magnetic effects will be observed. Connect the other extreme plate and the middle one with the galvanometer, and the needle will be powerfully deflected in the opposite direction.

This appears to offer the true explanation of the secondary piles of Ritter, and the more recent experiments of M. De la Rive.

10. When diluted sulphuric acid is employed in an elementary battery, the water is rapidly decomposed, and hydrogen is copiously evolved at the surface of the copper plate, even when a diaphragm of moistened bladder is interposed between the plates. With this acid the electro-magnetic effects are proportioned to the quantity of hydrogen liberated at the copper plate, without any regard to the immense quantities which may be liberated at the surface of the zinc plate. When nitric acid is employed, a much greater electro-magnetic effect is produced, though a much less quantity of hydrogen be now liberated at the surface of the copper plate. This acid seems to favour the facility of the definite arrangement of the molecules of water, without rendering it so easily decomposed. When the surfaces of the zinc and copper plates are covered with bubbles of hydrogen, the effect must be much diminished, as

the gases are non-conductors of voltaic electricity. The increase of effect, then, which is gained by the addition of nitric acid, seems to result from this circumstance, whilst the sulphuric acid, by dissolving the oxide, keeps the surface comparatively clean. When diluted sulphuric acid alone is used, and the plates in an elementary battery placed at different distances from one another, the quantities of hydrogen disengaged at the surface of the copper plate are within certain limits inversely proportional to the square roots of the distance between the plates; a law which has been found to connect the distances of the plates with the electro-magnetic effects*.

If the plates be removed to a very great distance, they will be unable to arrange the molecules of the fluid in the definite order which seems essential to the development of voltaic electricity, when all action will, of course, cease. As we approach this limit, the diminution of effect goes on more rapidly than the square root of the distance. When the plates, on the other hand, are brought very near each other, the increase of effect goes on more slowly than the square root of the distance, probably on account of the small space being partially filled with gaseous matter. Had these exceptions to the general law not taken place, there would have been no limit to the increase of effect till the plates had been brought into actual contact; nor would there have been a complete destruction of voltaic effect till the plates had been removed to an infinite distance.

PART II.

INVESTIGATION OF THE FUNDAMENTAL PRINCIPLE AND LAWS OF ACTION OF THE VOLTAIC BATTERY.

11. In every theory of the battery which has yet been proposed, an actual transfer of electricity is supposed to take place, and a continued circulation kept up through the entire circuit. According to the two theories, the battery is supposed to be charged before the poles are connected, and the electricity thus accumulated is ready to rush along the connecting wire the moment the poles are brought in contact. I can find no proof either for this accumulation or actual transfer, nor have we any proof that voltaic action takes place till the circuit be completed. Nor does any theory of the battery which has yet

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* Journal of the Royal Institution, No. 1. New Series.

been proposed take into view the law which connects the voltaic action of an elementary battery (or that consisting of a single pair of plates,) with the distance between them; a law essentially connected, not only with the action of the battery, but with its very existence. It is curious to remark that VOLTA, by reasoning from false principles and on very imperfect data, arrived at the invention of one of the most powerful instruments of research which genius has bequeathed to the philosopher.

I would not be understood, by this remark, to detract from the merit of the Italian philosopher. I have ventured this observation to encourage the young philosopher in his pursuit of physical truth, even when his views are imperfect and obscure.

From these observations it is obvious that the theory of the battery is incomplete, if not absolutely false. It is entirely from possessing the most perfect measurer of voltaic electricity,—namely, the torsion galvanometer,—that I have been enabled to give a more complete analysis of the principles of the battery, and the laws which regulate the accumulation of voltaic power.

12. In analysing the compound effect of the battery, we must first examine what takes place, when a single pair of zinc and copper plates are soldered together, and diluted acid placed in cells on their opposite sides, instead of being placed between them as in the elementary battery.

Let *z c* (fig. 3.) represent a zinc and copper plate soldered together, and let C′, C″ be two copper plates of the same size connected together, and cemented in a trough having the cells filled with diluted acid. The acid in the left-hand cell, between the two copper plates, can act only as a conductor, and hence the action of the compound plate *z c* will be exactly the same as what would take place if C″ and C′ were connected by a fine metallic wire having the same conducting power as the mass of fluid contained in that cell. In order to ascertain the effect of this arrangement, let wires proceeding from the copper plates C′, C″ be connected with a very delicate torsion galvanometer having astatic needles. Let another compound plate of zinc and copper be cemented between the extreme copper plates, and let copper wires from these plates be connected with the galvanometer as before, and the deflecting force will be found to be doubled. If three plates be introduced, the effect will be tripled; and so on in proportion to the number of plates. Hence the voltaic effects of two batteries of the same length, and having the same size of plates, will be directly proportional to the number of plates. Hence it follows that each pair of plates produces an equal effect in whatever part of the battery it is placed. This is one of the fundamental principles on which the true theory of the battery is founded.

13. Since each pair of plates, then, produces an equal effect, let us now examine what will take place with regard to batteries of unequal lengths, when the plates are of the same size, and placed at the same distance from one another.

Let A, B be two batteries, having the same size of plates, and placed at the same distance from one another, and let n be the number of plates in A, and N the number in B. Let the voltaic effect of the extreme pair of plates in the first battery be denoted by F, and that of the extreme pair in the second by f.

Two equal plates of copper c, c are placed at the ends of each, and the extreme cells filled with the same diluted acid as that used in the other cells. The extreme pair in the battery A which produce the effect F, are $z' c'$; and those in the second battery which produce the effect f, are $z' c'$.

Now, since the effects of the extreme plates are inversely as the square roots of their distances, they will be inversely as the square roots of the number of plates. Hence $F : f :: \frac{1}{n^{\frac{1}{2}}} : \frac{1}{N^{\frac{1}{2}}}$. Multiplying the terms of this proportion by those of the identical proportion

$$n : N :: n : N,$$

we have $$nF : Nf :: \frac{n}{n^{\frac{1}{2}}} : \frac{N}{N^{\frac{1}{2}}}$$

or $$nF : Nf :: n^{\frac{1}{2}} : N^{\frac{1}{2}}$$

But n F being the accumulated energy of the battery A, and Nf that of the battery B, we have, within certain limits, the voltaic energies of two batteries, very nearly proportional to the square roots of the number of plates.

14. Had the conducting power of acid solutions in an elementary combination been in the simple inverse ratio of the distance, there could have been no accumulation of voltaic effect; or, in other words, the battery could never have existed.

For in that case we should have had

$$F : f :: N : n$$

and $n : N :: n : N$

Hence $nF : Nf :: nN : nN :: 1 : 1.$

That is, the effects of batteries of any number of plates would always have been to each other in a ratio of equality.

15. Had the law of diminution followed the square of the distance instead of the square root, there would obviously have been a loss of power by an increase in the number of plates. It is obvious, then, that any theory which does not take in the law of conduction must be founded on very imperfect data.

16. I was now anxious to ascertain whether the preceding reasoning was borne out by direct experiment, which must always be considered as the criterion of the truth of any theory in physical science. By the following experiments this theory of the battery must either stand or fall.

Exp. V. Having fixed two pieces of copper, an inch broad and two inches high, in the bottom of a box separated into two compartments by a diaphragm of bladder, and inverted a funnel-mouthed tube over one of them, (the box being previously filled with water slightly acidulated,) I connected the plates with the ends of a battery of thirty pair of plates. The hydrogen disengaged in three minutes was found to occupy about two inches and a half of the tube. The plates were now connected with one hundred and twenty plates of the same battery, and the decomposition allowed to go on for the same time; when it was found that the hydrogen collected was scarcely double that in the first experiment. Now the number of plates being as one to four, and the quantity of hydrogen nearly as one to two, we have the effects nearly as the square roots of the number of plates. By increasing the number of plates to a great extent, we should find, agreeably to a remark in § 10 of Part First, that the increase of effect would not go on so rapidly as the square root of the number of plates. This fact, which follows from theory, is strikingly confirmed by direct experiment.

17. The theoretical views now unfolded are strikingly confirmed by the application of the torsion galvanometer in the following experiment.

Exp. VI. Two copper plates four inches square, having copper wires soldered to them, were immersed in the extreme cells of a battery of thirty, four-inch plates; metallic contact between the copper plates and the ends of the battery being carefully avoided. The connexion being made with the cups of the galvanometer, the deflecting force was found to be equal to 90 degrees of torsion of the glass thread. The copper plates were then immersed in the extreme cells of the same battery, containing one hundred and twenty pair of plates, when it was found that the degrees of torsion were somewhat less than 180. Hence the electro-magnetic effects of the two batteries were nearly as the square root of the number of plates. Hence the electro-magnetic effects of two batteries are within certain limits proportional to the quantities of water decomposed.

18. These principles will enable us to account for a fact in electro-magnetism which has never been explained in a satisfactory manner. Since the discoveries in electro-magnetism, it was observed that no increase of electro-magnetic power is gained by increasing the number of plates in a battery, when the extreme plates were connected by metallic contact with the ends of the battery. This unexpected result has been accounted for by supposing that voltaic electricity, having tension like common electricity, acts feebly on a magnetic needle. Not feeling satisfied with this vague explanation, I had again recourse to experiment.

Exp. VII. Having soldered copper wires to several plates in a common galvanic trough, I connected a single pair with the galvanometer, and observed the deflecting force. By connecting the extreme plate and the others in succession with the galvanometer, the effect, within certain limits, was observed to be nearly constant. When the number of plates was increased to forty or fifty, a slight diminution of power was observed.

Since the deflecting force of a single pair is inversely as the square root of the distance between them, which is the whole length of the battery, and since the effects of all the other plates are directly as the square root of their number, it follows that within narrow limits the compound effect must be constant*.

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* The supposed analogy between common and voltaic electricity, which was so eagerly traced after the invention of the pile, completely fails in this case, which was thought to afford the most striking resemblance.

19. The law now established may be exhibited to the eye by the co-ordinates of a parabola. Let V B (fig. 5.) represent the length of a battery, and the ordinate B C its voltaic power; and let V E be the length of another battery, the plates being equal and placed at equal distances, and let D E be its voltaic power; then will F G represent the power of a battery whose length is V F. When the length of the battery becomes great, the length of the ordinates diminishes more rapidly than in the parabola, or the curve approaches more to the nature of an ellipse. For want of a battery of a sufficient number of plates, I have been unable to determine the real nature of the voltaic curve. It appears exceedingly probable that the curve returns into itself, or, in other words, that the battery after gaining a certain power gradually loses its energy by any further increase of the number of plates. I should have scarcely any hesitation in touching the ends of a battery a mile long, and still less if it were extended to the length of ten miles.

PART III.

APPLICATION OF THE PRECEDING PRINCIPLES TO FURTHER DEVELOPMENTS IN VOLTAIC ELECTRICITY.

20. There are only three substances which can be regarded as good conductors of voltaic electricity: viz. the metals, charcoal, and acidulated water. The metals when employed as conductors seem to have been the only substances whose deflecting energy on the needle has been carefully examined. It appeared to me worth an experiment to ascertain if charcoal deflected the needle, and that, too, with the same energy as metal conducting an equal quantity of voltaic electricity. This was ascertained by the following arrangement.

Exp. VIII. A piece of charcoal about an inch and a half long was placed between two slips of copper fixed perpendicularly in a piece of wood, and an astatic needle, suspended by a fibre of silk, brought over the middle of the charcoal. Fig. 6. will exhibit this arrangement, in which A B is the charcoal, c, c' the slips of copper, w, w' wires soldered to the copper slips. When the wires were connected with an elementary or with a compound battery, the needle was deflected in the same manner as by a metallic wire. When one of the slips of copper was placed below the needle and at the same distance as before, the needle was equally deflected. The needle is therefore deflected by the quantity of electricity conducted, without any regard to the nature of the conducting substance.

Since charcoal therefore deflects the needle, it might be made to rotate about a magnet, agreeably to the laws first established by the ingenious experiments of Mr. FARADAY. This was accomplished as follows:

EXP. IX. A thin slip of copper, having a small cup soldered on the middle, and two short tubes on the ends to hold pieces of charcoal, was placed on the point of a needle on the pole of a horse-shoe magnet, the points of charcoal being made to dip into the mercury contained in a wooden cup, as in the experiment for the rotation of a wire. The charcoal was now made to conduct voltaic electricity from a battery of a hundred pair of plates, when it revolved rapidly, as a metallic wire would have done in similar circumstances.

21. The French philosophers have introduced a distinction between the current of voltaic electricity passing along a metallic conductor, and that transmitted by a liquid conductor, which seems to me entirely groundless. This distinction may be given in the words of Mr. CUMMING in his translation of DEMONFERRAND's Manual of Electro-Dynamics: "In the present state of science it is perhaps expedient to consider electrical currents in another point of view, namely, as being continuous or discontinuous. The first are those which are transmitted by perfect conductors, and whose intensity varies insensibly in two consecutive instants; as in the thermo-electric or in the common galvanic circuits. When the conductors are imperfect, the currents are discontinuous: for bodies of this description permit the electricity to accumulate for a certain time, after which, the insulating force being overcome, it passes with an explosion; and if the electro-motive power continues to act, there ensues a second accumulation and explosion as before, and so on successively. The distinctive character of such currents is, that they are incapable of producing a deviation in the magnetic needle*."

From the manner in which this distinction is laid down by the French writers, I had always taken it for granted that a needle suspended above the liquid part of a conductor of voltaic electricity was feebly deflected, and should probably have remained in this belief had I not entered on the present experimental investigation. This assertion I put to the test of experiment, as follows:

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* CUMMING's Translation of DEMONFERRAND's Electro-Dynamics, p. 116.

Exp. X. Having cemented a glass tube, about an inch in diameter and four inches long, into two wooden boxes A, B, as in fig. 7, and filled the whole with water, I placed two plates of copper *c*, *c'*, having copper wires soldered to each, opposite the ends of the glass tube T. The wire proceeding from *c'*, after extending about a foot upwards, was bent towards the left at D, and then made to descend at E and pass parallel to the needle N S, and thence to the end of a battery. The other wire *w* was connected with the other end of the battery. It is obvious from this arrangement, that the effects of the electricity arranged in the cylinder of water, and those of the horizontal branch of the wire above the needle, would be to turn the needle in opposite directions. When the branch G H was further from the needle than the axis of the tube, represented by the dotted line, the needle was deflected in obedience to the cylinder of water; but when the wire was brought nearest the needle, it was deflected in the opposite direction.

When the wire was placed at the same distance from the needle as the axis of the cylinder, the needle remained perfectly stationary. When a disc of zinc was substituted for one of the copper plates, and the wires connected so as to form an elementary battery, the needle was deflected with the same force by the column of water as by the metallic part of the circuit. Hence it is obvious that a cylindrical column of water conducting voltaic electricity deflects the needle with the same energy as a metallic wire passing along its axis and forming a part of the same circuit.

22. To complete this part of the inquiry, I was anxious to make a hollow column of water revolve about the pole of a magnet. This was accomplished as follows:

Exp. XI. Having procured two thin hollow cylinders of wood, the one about two inches and a half in diameter, and the other about an inch and a half, I cemented the one within the other at the bottom. Two flat rings of copper were then fixed parallel to each other, at the bottom and top of the cylindrical box, the space between them being for the reception of water or diluted acid. This annular space was divided into two compartments by thin slips of wood, placed perpendicularly to prevent the water revolving without carrying the box along with it. Two small metallic points were made to pass from the lower copper ring through the bottom of the box, for the purpose of dipping into a cup for holding mercury. The opposite sides of the upper ring were connected by a wire, to the middle of which was soldered a metallic point to dip into a small metallic cup for holding mercury on the top of the magnet. The whole arrangement will be obvious from the simple inspection of fig. 8, in which the cylinder C is seen in perspective, with the metallic point to dip into the mercury contained in the wooden cup which is used for the common rotation of a wire, and N the magnet. The box is partly suspended by an untwisted thread T, so that the metallic point may not rest on the bottom of the cup on the top of the magnet, but simply dip into the mercury.

When the wire *w*, from the cup on the top of the magnet, is connected with one end of a powerful battery, and the wire from the wooden cup into which the metallic point dips, connected with the other, the water in the box is rapidly decomposed, and the whole revolves about the pole of the magnet. By changing the poles, the box and its contents are made to turn round in the opposite direction. I was now anxious to make the hollow cylinder of water revolve, whilst the vessel in which it was contained remained stationary. This was accomplished by the following arrangement.

Exp. XII. Two glass cylinders were cemented into grooves in a circular piece of wood, through the centre of which the magnet was made to pass, as in the preceding experiment. The circular rims of copper were fixed as in the wooden cylinder, the breadth of the upper ring being considerably less than that of the lower. The inspection of fig. 9. will render the whole obvious, in which A B is a section of the glass cylinders, N the magnet, W the wire connected with the lowest copper ring, W′ that connected with the other and reaching to the ends of the battery; V represents a wooden vane, having two vertical branches immersed in the conducting liquid, and balanced on a fine point resting on the top of the magnet.

When the wires are connected with a powerful battery, the water begins to revolve, forming a real vortex and carrying the wooden vane along with it. When bodies of the same specific gravity as that of the fluid are thrown into it, the rotation is rendered obvious without the wooden vane. When the lower ring is connected with the negative end of the battery, the bubbles of hydrogen as they ascend wind round in a spiral direction till they reach the surface of the fluid.

These experiments demonstrate that the action of magnets is entirely on the electric current or electric arrangement, without any relation to the ponderable substances with which it combined; and they may yet enable us to assign the causes of currents in the ocean which have not received a satisfactory explanation.

23. In examining the changes which take place in water placed between the platina poles of a powerful galvanic battery, I was struck with the difference of temperature which I observed in the water at the two poles. The phenomena which thus presented themselves appearing to me new and highly interesting, I was induced to examine the subject by careful experiments and investigation. The following arrangement presented itself, and brought out new and unexpected results, which seem to open a wide field for future inquiry.

Exp. XIII. Having made a small rectangular box, I divided it into three compartments by diaphragms of bladder, as in fig. 10, in which A, B, C are the three chambers. Platina poles were introduced into the extreme chambers, and the box nearly filled with common water. The copper wires *w*, *w'* being connected with the ends of a powerful battery, the water was rapidly decomposed through the moist bladder. After decomposition had gone on for eight or ten minutes, the temperature of the water in the three cells was examined, when it was found that the temperature of the water in each of the cells had risen during the experiment, that the temperature of the water at the positive pole had risen several degrees higher than that in the negative cell; but what seemed most remarkable was the fact that the water in the middle cell had risen several degrees higher than the water in the positive or hottest chamber. The cause of this curious result soon presented itself.

The general rise of temperature in the conducting fluid is undoubtedly caused by the same agency which raises the temperature of a metallic wire performing the same office as the fluid. The difference of temperature in the extreme cells depends on the specific heats of the gases disengaged. The specific heat of oxygen is nearly the same as that of hydrogen. But there being twice as much hydrogen given off at the negative pole as oxygen at the positive, it will absorb nearly twice as much heat from the water in that chamber as the oxygen does from the water in the other. The temperature of the water in the negative chamber must therefore be kept lower than that in the other compartment. If liquids could conduct voltaic electricity without suffering decomposition, the temperature of the whole mass between the poles would have its temperature equally elevated in every point; but the two unequal cooling processes going on in the extreme chambers, occasion the striking inequality of temperature in the three divisions. But this experiment appears to me to establish another point of vast importance in the theory of voltaic electricity. If the hydrogen which is set at liberty at the negative pole traversed the fluid between the two poles, it is obvious it must have acquired its specific heat at the positive pole, and consequently could not have lowered the temperature in the negative cell. The oxygen, then, which is disengaged at the positive pole must have belonged to the film of water in contact with that pole, and the hydrogen set at liberty at the negative pole must have been the hydrogen belonging to the film of water in contact with the negative pole. There appears, therefore, to be no actual transfer of the component parts of water, but, agreeably to the views of M. Grotthus, a continued series of decompositions and recompositions along the whole chain of aqueous particles between the two poles.

24. The explanation now given of this curious phenomenon receives the strongest confirmation from the decomposition of other substances besides water. When a solution of sulphate of copper was placed between the poles of a powerful battery, and the temperatures of the cells examined as before, it was found that a much greater difference between the temperatures of the extreme chambers took place; but the temperature of the negative chamber was now higher than that of the positive. In some of my experiments the temperature of the negative cell rose eight or ten degrees above that of the positive, whilst the middle chamber was nearly of the same temperature with the negative compartment. The same striking difference was observed when a solution of acetate of lead was employed.

The cause of this change of temperature depends, as in the case of water, on the specific heats of the elements separated at the two poles. When a metallic salt is decomposed by the agency of voltaic electricity, the pure metal is separated at the negative pole, whilst the oxygen appears at the other pole. Now, the specific heats of metals are exceedingly small. The change of state, then, from the liquid to the solid, which took place in the negative chamber, must have raised the temperature, whilst no such change of state took place in the positive compartment. Hence the temperature of the negative cell must be higher than that of the positive. It is unnecessary to multiply examples. If we know the specific heats of the substances set at liberty in the extreme chambers, we can tell, à priori, which of the compartments will have the highest temperature,—which affords the most satisfactory evidence of the accuracy of the explanation given of these interesting phenomena.

XIV. *Of the Organs of the Human Voice.* *By Sir* CHARLES BELL, *K.G.H. F.R.S. L. & E. &c. &c. &c.*

Read February 2, 1832.

THE organs of the Human Voice are related to many interesting inquiries in science and philology; and yet it is remarkable that this subject has hitherto occupied no place in the Transactions of the Society. In a matter so open to observation as the anatomy of the throat, there can, indeed, be no new parts discovered; but it will be easy to show that their actions have been very negligently treated.

It will not, I hope, lessen the interest of the inquiry, that I acknowledge having an ulterior object in it. The nerves distributed to the neck and throat are the most intricate of all. That they have not been unravelled, and distinct uses assigned to each, is owing to the complexity and the numerous associations of the organs to which they tend. When we shall have seen the necessity of combination among the various parts, for producing the simplest effort of the voice, we shall find a reason for these numerous nerves, and for their seeming irregularities.

In reviewing the writings of physiologists we observe defects which are obviously to be ascribed to the great complexity in the organization, and the real difficulty of the subject: but there are others which arise from the habit of resting contented with assigning one use for a part in the animal frame; whereas there is nothing which should more excite our admiration, than the variety of offices destined to be performed by the same organ. It is in contemplating the extent of combination established among the parts of the human body, that we become sensible of its perfection above all comparison with things artificial; and this is especially true with regard to the organs of the voice. They are remarkable for their union or cooperation in function; they all perform more than one office, and are interwoven and associated with parts which serve a double or even a treble function. But we ought not to be surprised at the intricacy of structure in the human organs of voice, when we find them capable of imitating every sound of bird or beast, excelling all instruments of music in clearness and expression, and capable of making those infinite changes on articulate sounds, which form the languages of the different nations of the earth.

Although there be one subject,—Articulate language,—on which I shall principally comment, as being that in which the treatises on the voice are altogether defective; yet, as there are lesser points in which I think authors are in fault, I shall take the subjects consecutively or systematically. I do this in the hope of affording, at the same time, a sounder foundation in anatomy, to those members of the Society who are more capable of pursuing this part of philosophy in all its curious and elegant subdivisions.

It will be convenient to divide the inquiry into three heads:—the *Trachea*, the *Larynx*, and the *Pharynx*.

Under the head of *Trachea*, and through the whole investigation, it is necessary to keep the different functions of the part in mind; or we shall be appropriating to the voice, structures which have reference to other functions. We read that the trachea is formed of imperfect hoops of cartilages, joined by membranes, and that it is flat on the back part, for these reasons: that it may be a rigid and free tube for respiring the air—that it may accommodate itself to the motions of the head and neck—and that it may yield, in the act of swallowing, to the distended œsophagus, and permit the morsel to descend. This is perfectly correct; but there is a grand omission. Whilst all admit that a copious secretion is poured into this passage, it is not shown how the mucus is thrown off.

There is a fine and very regular layer of muscular fibres on the back part of the trachea, exterior to the mucous coat, and which runs from the extremities of the cartilages of one side to those of the other*. This transverse muscle is beautifully distinct in the horse. When a portion of the trachea is taken out, and everything is dissected off but this muscle, the cartilages are preserved in their natural state; but the moment that the muscular fibres are cut across, the cartilages fly open. This muscle, then, is opposed to the elasticity of the cartilages of the trachea. By its action it diminishes the calibre of the tube, and by its relaxation the canal widens without the operation of an opponent muscle.

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* See Plate X. fig. 3. A.

The whole extent of the air-passages opens or expands during inspiration; and then the trachea is also more free; but in expiration, and especially in forcible expectoration and coughing, the trachea is diminished in width. The effect of this simple expedient is to free the passage of the accumulated secretion; which, without this, would be drawn in and gravitate towards the lungs. When the air is inspired, the trachea is wide, and the mucus is not urged downwards; when the air is expelled, the transverse muscle is in action, the calibre of the tube is diminished, the mucus occupies a larger proportion of the canal, the air is sent forth with a greater impetus than that with which it was inhaled, and the consequence is a gradual tendency of the sputa towards the top of the trachea. In the larynx, the same principle holds; for as the opening of the glottis enlarges in inspiration, and is straitened in expiration, the sensible glottis, by inducing coughing, gets rid of its incumbrance. Without this change of the calibre of the trachea, the secretions could not reach the upper end of the passage, but would fall back upon the lungs.

Experiments have been formerly made*, which, although no such view as I now present was in contemplation, prove how the action of the transverse muscle tends to expel foreign bodies. The trachea of a large dog being opened, it was attempted to thrust different substances into it during inspiration; but these were always sent out with impetus, and could not be retained. Why the dog could not be thus suffocated is apparent; the tube is furnished with this most salutary provision to secure the ready expulsion of all bodies accidentally inhaled; the air passes inwards, by the side of the foreign body; but in its passage outwards, the circumstances are changed by the diminished calibre of the canal, and the body, like a pellet filling up a tube, must be expelled by the breath.

Looking on the form and muscular structure of the trachea in man, as providing for expectoration of the secretions poured into the tube, what shall we think of the tracheæ of birds, which are formed by cartilages of complete circles, and which have no compressing muscles? Does it explain the peculiarity, that all the air-tubes of birds are dry; that their lungs are motionless; and that in the air respired by them there is no moisture?

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* By M. Favier.

These are the reasons why I must reject the opinion of PORTAL, that the transverse muscle of the trachea is to give force to the breath in speaking.

The trachea, and all that portion of the windpipe which extends from the larynx to the lungs, may be considered as the porte-vent, or tube which conveys the air from the bellows to the reed of the organ-pipe; and it has even less influence on the quality of sound than the porte-vent. If this portion of the air-tube were to vibrate and give out sound, it would interfere with, and confuse those which proceed from the glottis. The imperfect circle formed by the cartilages of the trachea, and their isolation from each other, are ill suited to convey sound.—But I am now to notice a more particular provision against the propagation of sound downwards by this passage.

If on inspecting a musical instrument we should find a spongy body of the consistence of firm flesh in contact with a cord or tube, and an apparatus by which this body might be pressed against the vibrating part, we would not hesitate to conclude that it damped or limited the vibration. The THYROID GLAND is a vascular, but firm substance, which, like a cushion, lies across the upper part of the trachea*. Four flat muscles, like ribbons, arise from the sternum, first rib, and clavicle, and run up to the thyroid cartilage and os hyoides, over the surface of this glandular body. These muscles are capable of bracing it to the trachea. If it be admitted that the vibration of the trachea would only produce a continued drone, rising over the inflections of the voice and adding nothing to its distinctness, we may perceive in the adjustment of the thyroid gland to the trachea the most suitable means of suffocating or stopping the vibrations from descending along the sides of the tube.

Comparative anatomy is often a test of the correctness of our inferences drawn from the human body. I reflected that if I were right in my idea of this being one of the uses of the thyroid gland, there should be no such body, so placed, in birds: and that, following up the inquiry, if we were not likely to discover the function of that gland, we might nevertheless learn why it is so singularly placed. In birds the sounding apparatus is at the lower part of the trachea; the larynx being, in a manner, divided in its office. At the upper opening there is the structure, and action, and sensibility, constituting it a guard against foreign matter; but the proper organ of sound is formed on the lower extremity of the trachea and in the chest. Hence, in birds, there is this remarkable difference, that the sound must ascend along the trachea. Directed by this consideration, it is not without interest that we notice the absence of the thyroid gland in them; that the trachea itself is a firm tube with cartilages of entire circles; and that there is nothing to suffocate the rising vibrations. In no animal is the thyroid gland of the same relative magnitude as in man.

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* See Plate X. fig. 1. D. D.

But it is easy to prove that the trachea has no influence upon the voice. Both in the open pipe or flute, and the pipe stopped at the bottom, as the syrinx, the length determines the note,—lengthening the tube depresses the note, and shortening it makes the sound more acute. A similar effect should result from the elongation and shortening of the trachea, if the changes of the voice depended upon it: but, on the contrary, the trachea is lengthened during the high note, while it is shortened as the voice descends, and the notes become graver*. I have no ear to determine what harmonic sounds attend the human voice; but supposing that sounds proceed from the trachea, which is shortening, at the same time that they proceed from the upper part of the tube, which is lengthening, it is clear to demonstration that the two portions of the tube can never consent or keep any proportion in their vibrations.

For these reasons I apprehend that in the structure and condition of the trachea, the design manifestly is to suffocate the vibrations of sound, and so to impede the motions originating in the larynx from being propagated downwards.

Pursuing our inquiry into the organs of the voice independently of articulation, and looking more particularly to the *Larynx*, we shall find that the common opinion is confirmed by experiment and every analogy, that the glottis is the primary seat of sound—the source of the vibrations communicated to the air as it is breathed. But to consider the motions of the glottis, and even the modulations of the air in the larynx, as the sole source of sound, would be incorrect. **Ferrein** described the edge of the glottis as being like the strings of the violin, and the air brushing over it like the bow. But even in that supposition, though the vibration of the string of the violin is necessary to the production of sound, yet that sound receives modification through the form and condition of the instrument. As the same chord, vibrating in the same time, will produce a sound the quality of which varies in different instruments, so will the sound of the chordæ vocales be influenced in the pharynx. As a tuning-fork, or a moveable musical instrument, will have the quality and power of the tone changed by its position and the material with which it is in contact, so will the vibrations of the human glottis be affected by the parts above and against which the sound is directed.

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* FABRICIUS AB AQUAPENDENTE, seeing the contraction and elongation of the trachea during the changes of the voice, presumed that these motions must be the cause of them. DODART showed the incorrectness of this.

The breath, which plays inaudibly in respiration, becomes vocalized when the ligaments of the glottis, or chordæ vocales, are braced so as to cause the edges of the glottis to vibrate in the stream of air. In a wind instrument the air must be impelled with a force to make the sides of the tube vibrate; so, in the production of sound from the human organs, there must be a certain pressure of the column of air. But in the organs of the voice there is this superiority, that there are not only the means of regulating the pressure of the column of air, but of adjusting the vocal chords, so as to suit them to the most delicate issue of the breath. The metal tongue in the organ-pipe is, by lengthening or shortening it, accommodated so as to vibrate in time with the air contained in the tube. So is the edge of the glottis regulated; but with an apparatus for adjustment the most perfect.

Besides the adjustment of the vocal chords, there is a very superior provision in the motions of the chest which supply the air, to that of any musical instrument. Although the organ has allotted to each note a separate pipe, whose relative dimensions are proportioned with mathematical precision, yet the air propelled through the pipes can never be so regulated as it is by the combination which exists betwixt the motions of the chest and the glottis. The church organ could not be made to approach the precision of adjustment in the human organs, were there as many pairs of bellows as there are pipes, and each adjusted by a weight or spring, to accommodate the pressure of air to the dimensions of the pipes*.

Referring to the Plates for the anatomy†, I may continue my comment on the form and uses of the parts. The thyro-arytenoid ligaments, or chordæ vocales of FERREIN, are the lower ligaments of the glottis; they form the chink of the true glottis. These ligaments do not stand distinct from the sides of the tube, but the fine lining membrane is reflected over them. This membrane, sinking between the inferior and superior ligaments, forms there the sacculus or ventriculus laryngis. Another reflexion passes from the extreme point of the appendix of the arytenoid cartilage to the base of the epiglottis. These inflexions of the membrane of the glottis produce a considerable intricacy in the passage of the larynx. Nevertheless, when this piece of anatomy is fully displayed, the number of muscles inserted into the arytenoid cartilages, and the effect of their motions on the lower ligaments, point to these as the chief parts, and to the others as subordinate, in producing sound.

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* Which is attempted in some automata.

† See Plate IX.; and Plate X. fig. 2.

There are, however, circumstances which lead to the belief that the sacculus or lateral cavity of the larynx has much influence on sound. We perceive that one effect of this cavity is to hold off the inferior ligament from the side of the tube, and to give freedom to its vibrations. But the varieties in its size and form, exhibited by comparative anatomy, and the influence which some of the muscles of the arytenoid cartilages* must have upon it, point it out as an essential part of the organ of sound; and the ear-piercing cries which belong to such animals as the Beelzebub ape, in which this cell is large, confirm the notion.

The seat of the vibrations which produce the voice is so fairly indicated by the whole anatomy, and confirmed by observation, that there is hardly an excuse for those experiments which have exhibited the motions of the chink of the glottis in living animals†. It is, on the whole, better to wait our opportunity of inspecting these parts in action in man. In consequence of wounds of the throat, I have had repeated occasions to witness the motions of the glottis in man, both during simple breathing and in speaking. On every inspiration the glottis is dilated. Upon asking the patient to speak, and encouraging him, when no sound proceeded, by saying that I could understand him by the motion of his lips, I have seen that in the attempt at utterance, the glottis moved as well as the lips. Although these occasions be too painful to admit of protracted experiment, I could not omit observing that there is a motion of the glottis in correspondence with the efforts of the other organs of voice.

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* Thyro-arytenoideus and Crico-arytenoideus.

† The larynx of a dog being partially dissected, so as to expose the glottis, the experimenter tortured the animal to observe how the acuteness of the note, and the constriction of the chink of the glottis bore relation to the severity of pain. After ascertaining the degree of contraction from the pinch of the tail to the application of the red-hot iron, he set himself with a tuning-pipe to sound in harmony.—Archives Générales de Medecine, tom. xxv. Mars 1831.

We have already understood the necessity of the tongue of the organ-pipe being adjusted in its length, both to the force of the wind from the bellows, and that it may vibrate in correspondence with the column of air in the tube. Granting that the analogy between this instrument and the organ of the voice is just, we must acknowledge the very superior means possessed by the living parts, of drawing out the margin of the glottis, to that by which the tongue of the organ-pipe is adjusted.

If we should adopt the fancy to compare the membrane which is stretched over the ligament to a drum, then the arytenoid muscles would be the braces to tighten the membrane, and the ligaments would be as the snares on the reverse of the drum. But all such comparisons serve to show that, taking this portion only of the apparatus for the voice, it surpasses every instrument in the property of accommodation—of sounding in unison with the rest of the tube, and with the column of air.

Of the Pharynx, and of the formation of articulate Sounds.

We come now to a division of our subject, which, notwithstanding its higher interest, has been imperfectly treated by authors, and where the actions essential to articulate language have been altogether omitted.

Tracing the volume of simple sound in its ascent from the glottis, we see how well the epiglottis is calculated to direct it on the passages above*. Immediately over the epiglottis hangs the velum palati; this curtain is formed by certain muscular fibres, which draw down the mucous membrane from the back part of the bony palate into a great fold; whilst other muscles, their opponents, furl it up. This velum forms a partition which divides the mouth from the posterior cavity, arrière-bouche, or pharynx; and the velum, uvula, and arches of the palate vary their condition during the production of simple sounds.

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* See Plate IX.

When the parts are displayed, so that we may look on the outside and posterior aspect of the great bag of the pharynx, we see how well it is adapted for the office which I shall assign to it in the formation of the human voice. It presents to our view a flat expanded web, of a fleshy or muscular texture, and it extends from the base of the skull to the extremities of the horns of the os hyoides and those of the thyroid cartilage, between which it is stretched and held out. Behind, its connexions are loose; and as it forms a principal boundary of the bag of the pharynx, the great cavity of that bag is directly in front of it. If we trace the pharynx upwards from the closed extremity of the œsophagus, we perceive the glottis opening into it below; whilst above, it is terminated by the posterior nostrils, and anteriorly by the mouth.

Considering the passage for the voice as one irregular cavity, extending from the glottis to the lips and nostrils, we shall find it subject to great changes, and powerful in its influence on the voice. For although the breath is vocalized by the larynx, both the musical notes in singing and the vowels in speech, are affected by the form and dimensions of this cavity.

Notwithstanding the ingenuity displayed in experiments on animals, to show that their cries proceed from the larynx, we have no authority to disregard the fact, that when a person who has divided the pharynx, and exposed the top of the windpipe, attempts to speak, no sound issues from the larynx. By great effort he may produce a noise; but anything like the common effort of speaking is attended with no audible sounds. From this we must infer that the delicate vibrations, necessary to articulate language, are influenced not merely by the action in the glottis, but by the condition of the walls of the pharynx; the cavity into which the sound is thrown.

In this part of the air-passage, we shall find an exact correspondence with the flute or pipe, in as far as it is lengthened during the grave sounds, and shortened in the acute. Even if it were proved that the note is made to rise and fall by the contractions of the glottis, the great apparatus employed to move the pharynx cannot be useless. We are countenanced in concluding, that as the tube of the organ is adjusted to the reed, so is the condition of the pharynx made to correspond with these contractions of the glottis. It is impossible to see a singer running up the notes to the highest, without admitting that there must be a powerful influence produced through the alternate shortening and elongation of the pharynx and mouth. To allow the cavity to be shortened in the greatest degree, the larynx is raised, and the lips retracted; on the contrary, the trachea descends, and the lips are protruded, to lengthen the cavities, and to give out the lower or graver notes.

Of Articulation.

In pronouncing the simple continued sounds, the vowels, and the diphthongs, which are the combinations of open sounds, the pharynx, at all times irregular, varies its form or dimensions, without interrupting or cutting the sounds. These sounds are universal and expressive. What we have now to consider are more conventional, and form the constituents of articulate language.

It has been imagined that the vocalized breath ascending into the mouth is there divided, and articulated by the tongue, teeth and lips; and that this comprehends the whole act of speech. Such a description implies a very imperfect acquaintance with the actions which produce articulate language.

It is now my purpose to show, that in articulating, or forming the consonants, the pharynx is a very principal agent; and that this smaller cavity is substituted for the larger cavity of the chest, to the great relief of the speaker, and the incalculable saving of muscular exertion.

The late Dr. YOUNG made a comparison of the power employed by a glass-blower, in propelling the air through his tube by the force of his cheeks, and in propelling it by the force of his lungs; and calculating the ease with which the lesser cavity is compressed in comparison with the greater,—that is, the cavity of the mouth compressed by the muscles of the cheeks, compared with the whole extent of the chest compressed by the muscles of respiration,—he concluded, that the weight of four pounds would produce an operation through the lesser cavity, equal to seventy pounds weighing on the larger cavity.

The quality of fluids, by which they transmit pressure equally in all directions, is the cause of this and of some other results which appear paradoxical. It is a property too nearly allied to mechanical power, and too important to be left out of the scheme of animal structure.

When a forcing-pump is let into a reservoir, it produces surprising effects. The piston of the hydraulic press being loaded with a weight of one pound, the same degree of pressure will be transmitted to every part of the surface of the reservoir, equal in magnitude to the base of the piston. And on the contrary, supposing the power to be employed on the reservoir for the purpose of raising the piston, it would require the weight of a pound on every portion of the superficies of the reservoir, equal in extent to the base of the piston, to raise the piston with a force of one pound.

We cannot fail to notice the effect of this law on the cavities of the animal body, in diminishing the power of muscular bags in proportion to their increased capacity.

Elastic fluids are subject to a similar influence, from the pressure extending in every direction, and the resistance always being equal to the pressure. A man standing on the hydraulic bellows, raises himself by blowing into the tube: and contrariwise, the weight of his body does not produce from that tube a blast of air superior to the force of contraction of his cheeks. A very slight pressure against the nozzle of the common bellows will resist the compression by the handles; and by blowing into the nozzle, we may raise a great weight placed on the boards. To reconcile us to the influence of this principle, as applicable to the animal economy, we shall take an example before applying it to our present subject.

A sailor leaning his breast over a yard-arm, and exerting every muscle on the rigging, gives a direction to the whole muscular system, and applies the muscles of respiration to the motions of the trunk and arms, through the influence of a small muscle that is not capable of raising a thousandth part of the weight of his body. He raises himself by the powerful combination of the muscles of the abdomen, chest and arms; but these muscles are controuled and directed by the action of a muscle which does not weigh five grains. The explanation is this;—a man preparing for exertion, draws his breath and expands his chest. But how is this dilatation to be maintained? if the muscles which expand the chest are to continue in exertion to preserve it so, there must be a great expenditure of vital force; besides, these muscles are now wanted for another office. The small muscle that closes the chink of the glottis suffices. It contracts on the extremity of the windpipe; and here, acting so as to confine the column of air, it is superior to the united power of all the muscles of the chest and trunk of the body which act upon the cavity of the thorax. However powerful the muscles of expiration may be in compressing the chest, their influence is very small on the column of air in the windpipe; the pressure there being no more than on any part of the walls of the chest, which is of the same diameter as the base of the tube. The closing of the glottis by this small muscle, throws all those of the chest and abdomen, which are otherwise muscles of respiration, free to act as muscles of the trunk and arms.

But if any defect of the windpipe, or of the muscle which closes it, permit the air to escape, the muscles of the chest and abdomen sink with the falling of the chest; they become muscles of expiration, and lose their power as muscles of volition, consequently all powerful efforts cease in the instant. When an unhappy suicide thinks to perpetrate self-destruction by dividing his windpipe, his sensations of sudden and total failure of strength announce the accomplishment of the act; but he is deceived. In the moment of lunatic excitement, his energies are wound up, and his breath is drawn and confined; but now the trachea being divided, in the instant he is seized with feebleness; for the compressed air is let loose, the chest subsides, and the whole muscles of the trunk and arms are lost to the actions of volition. He feels as if struck with the sudden influence of death; his actual death depends on other circumstances.

Thus we perceive that the muscle of the glottis, not weighing a thousandth part of the muscles of the trunk of the body, controuls them all; changing them from muscles of respiration to muscles of volition; and this it is enabled to do on the principle of the hydraulic press.

We are by these instances prepared to understand the great importance in the animal economy, of power being employed on the lesser cavity in preference to the larger*; and how much will be saved if the appulse necessary in articulation be given by the pharynx instead of by the greater cavity of the thorax.

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* The principle is as important in its application to pathology as to the natural functions. It explains the weak pulse which attends the dilated heart; how the contractions of the uterus become more powerful in the progress of labour; and why the distended bladder acts with diminished power in the expulsion of the urine through the urethra. On the same grounds we understand how a slight spasm in the canal of the urethra will resist the most powerful contractions of an enlarged and thickened bladder, aided by the pressure of the abdominal muscles.

In a person whom I had the pain of attending for a long time after the bones of the upper part of the face were lost, and in whom I could look down behind the palate, I saw the operation of the velum palati. During speech it was in continual motion; and when this person pronounced the explosive letters, the velum rose convex, so as to interrupt the ascent of the breath in that direction; and as the lips parted, or the tongue separated from the teeth or palate, the velum recoiled forcibly.

These facts lead us to the further contemplation of the pharynx. We see it to be a large cavity behind the palate, formed by a dilatable bag, and acted on by many muscles. We have seen that the volume of sound issues into it from the glottis below; and that although it opens into the nose above, yet this passage is closed, whenever the velum is raised, like a valve, in the manner just described; at such a time, if the mouth be also shut, the bag will be closed on all sides, and may then suffer distention by the vocalized breath ascending through the glottis.

In speaking, much of the sound, as of the vowels and diphthongs, is the uninterrupted issue of the vocalized breath, modulated by the passages, and differently directed, but not checked or interrupted. The consonants are the same sounds checked by the tongue, lips, or teeth. At the moment of this interruption, the pharynx, being distended, is prepared to give an appulse by its muscular action exactly in time with the parting lips.

If we grasp the throat whilst speaking, so that the fingers embrace the bag of the pharynx, we shall feel that each articulate sound is attended with an action of the pharynx; and preceding each explosive letter, we shall be sensible of a distention of the throat. By a close attention to the act of breathing, we shall perceive that whilst the distended chest falls gradually and uniformly, the bag of the pharynx is alternately distended and compressed in correspondence with the articulated sounds.

We can now conceive that if each appulse of the breath in speaking arose from the action of the chest, it would be attended with great and unnecessary exertion; since in proportion to the size of the reservoir and the smallness of the tube that gives issue, would be the force required on the sides of the reservoir to produce an impulse along the tube. If each consonant and accented syllable required the action of the whole thorax, we should find that a man, instead of being able to deliver an oration of some hours in length, would be exhausted in a few sentences; like a person who bellows and gives pain by the violence and consequent ungracefulness of his action.

If we enter into a more particular examination of the formation of the consonants, we shall perceive that, without the action of the pharynx, those letters must have been mutes, which, through its operation, do in fact give the greatest force and distinctness to language. The circumstance which I have to notice could not altogether escape the observation of grammarians. They speak of the guttural sounds as belonging to the production of certain consonants. Bishop WILKINS expresses this by referring to that *murmur* in the throat before the breath is emitted in pronouncing these letters. Thus grammarians distinguish the mute letter P, which has no sound previous to the parting of the lips, from B, which has a guttural sound before the explosion of the lips.

Had the cause of this sound been investigated, these ingenious men would have presented the subject to us in greater simplicity. "This guttural sound," they say, "is produced by a compression of the larynx or windpipe:" but this has no meaning, and cannot pass for an explanation. This murmur, like all other sounds, proceeds from the vibration of the glottis; but, as we have seen, the glottis cannot vibrate without the ascent of the breath through it;—how then is this murmur to be produced when the mouth is closed, and there is no aspiration? The air ascends because the bag of the pharynx, or arrière-bouche, is filling. It is during the distention of the bag, that the breath ascends and produces the sound which precedes and gives the character to some of the explosive letters; and it is this preceding murmur which distinguishes these letters from others, produced by the same position of the "organs" in the mouth, but which are mute or nasal. Thus the triad of consonants D, B, G (hard), are called semimutes, because, without the assistance of any vowel, they are attended with a faint sound, "which continues for a little time." The letters T, P, K are produced by the same position of the organs in the mouth, but they are preceded by no murmur; and therefore it is that they are called mutes: whereas, in D, B, G, the pharynx fills, preceding the parting of the lips. It is this filling of the pharynx, and consequent murmur in the glottis, which gives reason for the grammarians to say that these letters, D, B, G, are accompanied with a sound, though not joined to a vowel, and to call them semimutes.

Grammarians admit "that the mouth is not the proper organ for producing sound, but only the organ for modulating and articulating the specific sounds;" and having explained the formation of the vowels, they proceed to the formation of the consonants, accounting for their peculiar sounds by the position of the lips, tongue, and palate.

We perceive that their explanation must necessarily be imperfect, owing to their ignorance of the anatomy, and especially of the action, of the pharynx. For example, P, B, and M, they say, are consonants formed by the application of the lips to each other: but this leaves the peculiar character of each letter unexplained, since all three are formed by the lips. The real difference is this: P gives no sound previous to the parting of the lips; it is the vowel abruptly sounded by their separation. B differs only in as much as the sound precedes the opening of the lips in the manner I have just explained; and as the pharynx, after being distended, contracts and forces open the lips, this letter is very properly called explosive. M, too, is in part owing to the articulation through the lips; the sound, commencing in the vowel, is interrupted by the shutting of the lips; after which it continues in a murmur; with this difference from the guttural murmur,—that it ascends into the cavities of the face, the velum being lifted. The same difference is shown in other letters, as F and V. If we attempt to articulate certain letters in a whisper, we shall find how much the distinctness depends on the swelling of the pharynx. In a whisper it is with much difficulty that we can distinguish P from B, or T from D, or G (hard) from K.

Thus we see that the consonants, classed according to their formation in the mouth, have varieties consequent on the action of the pharynx. 1st, The consonants formed by the closed lips; 2nd, Those formed by the meeting of the lips and teeth; 3rd, Those formed by the tip of the tongue and palate; 4th, Those formed by the dorsum of the tongue and palate. All of these admit of variety by the operation of the pharynx and velum; viz. they are mutes, explosive semimutes, and nasal liquids. For example, taking the position of the tip of the tongue against the teeth as forming a consonant, we have T, the mute; D, the semimute, in which the sound precedes the explosion; and N, the sound which rings through the nasal cavities after the closing of the passage through the mouth.

From the same misconception of the actions which combine to form the voice, it may be, that grammarians do not give us a very clear account of emphasis and accent. We perceive that there are two sources of the force with which the words are uttered,—the chest, and the pharynx. The emphatic delivery of several words or syllables must proceed from the forcible expulsion of the breath by the effort of expiration; but the emphasis on the single syllable, and the forcible enunciation of the letter on which the clearness and distinctness, and sometimes the meaning, of words depend, must be produced by the effort of the pharynx.

Proofs of the Correctness of the Opinions advanced, drawn from the effects of accident and of disease occurring under the Author's observation.

1. A child having drawn the broken shell of an almond into its windpipe, was in momentary danger of suffocation; and could utter no sound until the shell was extracted by incision*.

2. Owing to disease of the glottis, it was necessary to open the membrane between the thyroid and cricoid cartilages; the voice instantly ceased; and no sound could be produced, while the air passed freely from the wound: "the harsh sawing sound of the air in the contracted glottis immediately ceased, and the air played easily with a siffling sound through the wound."

3. A small pebble having fallen into the glottis of a child, there was a stridulous sound in drawing the breath, but no voice in the expulsion of the breath.

4. When an ulcer had destroyed the margins of the glottis, and the sacculi, the patient spoke in a husky whisper, "reedy and very feebly."

5. Thickening of the membrane of the glottis and epiglottis had a similar effect, the person speaking painfully in a whisper.

6. A man died of suffocation from a pustule, which formed on the margin of the false glottis; whilst he breathed, the sound was like the noise of a saw, harsh and loud.

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* The probe was passed several times into the windpipe, and past the broken shell without discovering it. It had been caught by the action of the transverse muscle, and the sharp broken edge forced into the mucous membrane; which was the reason that it was not coughed out of the wound.

7. The epiglottis being destroyed, and a deep ulcer in the sacculus, "the man attempted to call, but with a husky sound."

8. When the interior of the larynx was coated with coagulable lymph, except the clangour, during coughing, the voice was quite gone.

9. When the suicide has divided the larynx from the tongue, and opened the pharynx, no sound issues from the larynx in his attempt to speak; and it requires a powerful effort to produce any sound at all. When the glottis is thus exposed, it is seen to move in the effort to speak.

10. The loss of the velum pendulum palati was attended with the defect of articulation; the sounds were run together and nasal.

11. When polypus fills the cavities of the face, the voice is deficient in sonorousness and clearness.

12. When a communication is formed between the mouth and nose, the sound is nasal, and the articulation imperfect.

13. The entire removal of the bones of the face deprived the voice of all force, and gave it a sound which we should have called nasal, had any part belonging to the nose remained.

14. The defect of nervous influence in depriving the muscles of the velum and pharynx of due tension (as in apoplexy,) produces stertor or snoring. That this depends in a great measure on the relaxation of the velum, appears from this,—that changing the position of the head, so that the velum shall not hang against the back part of the pharynx, removes the distressing sound.

15. In extreme weakness, as from wounds and loss of blood even to insensibility, groaning proceeds from the condition of the glottis; as if the call for sympathy and assistance were intended to be the last effort of life.

By these facts it appears; 1st, That the trachea gives out no sound of itself; 2nd, That when the passage of the trachea is much encroached upon, the column of air is not sufficient to move the cords of the glottis; 3rd, That whatever interferes directly with the motion of the glottis, reduces the voice to a whisper; 4th, That when the larynx is separated from the pharynx, delicate sounds are not produced; and therefore an influence of the pharynx upon the stream of air is necessary to the production of such sounds; 5th, That any permanent opening or defect of the velum, which shall prevent the distention of the pharynx and the closing of the passage to the nose, renders articulation defective; 6th, That the removal of the cells of the face, equally with their obstruction, deprives the voice of its body and clearness; 7th, In nervous relaxation of the muscles of the throat, there is sound; but its nature evinces how much the proper action of the muscles is necessary to the voice.

Recapitulation.

It is curious, and not without its use, to observe how many parts must conform, and how many actions must accurately correspond, to produce the simplest sound; and how many additional combinations there must be for the formation of articulate voice.

As we may audibly breathe through a trumpet without producing a note of music, so we breathe without the tremor of the glottis to produce voice properly, but only the whisper. To vocalize the breath, there must not only be a certain strength of impulse in the column of air, but there must be an adjustment of the vocal chords in the glottis. The mere impulse of the breath, however forcible, as in sneezing, does not necessarily move the chords of the glottis.

The chordæ vocales being strung by the action of their muscles in correspondence with the forcible expulsion of the breath, they vibrate: this vibration is reverberated on the column of air; and by an adjustment of the passages above, there is a correspondence between the motions of the glottis and the vibrations of the column of air. The breath, thus vocalized, forms the several open sounds or vowels by the change or modulation of the passages: for by the more or less contraction and dilatation of the tube, these sounds are modified; the vibrating air being differently directed, and impelled against different portions of the tube.

The musical notes are in the same way produced by changes in the force with which the voice is propelled, the degree of tension in the chordæ vocales, and the modulation or change in the form of the open passages. There is nothing more surprising than the precision with which the notes of the human voice are produced, as when we hear it rising above the sound of the church organ, the notes more liquid and distinct, and descending in a solfeggio of notes and half-notes, as if each arose from a different pipe, or were struck on a distinct instrument. Yet these falls are consequent on muscular action, which alters the diameter and form of the glottis, and the length and diameter of the pharynx. This minute accommodation of action does not merely evince the perfection of the organ, but shows a most surprising command possessed over it: and in this respect the muscular apparatus of the throat does not yield in comparison with that of the eye itself.

Struck with the perfection of the human voice, its precision, expression, and variety excelling the finest instruments mathematically constructed, we have more to admire in the production of those conventional sounds which become the instruments of thought and the source of all we know. Articulation results from a still more complex action of the organs of voice. In speaking, the voice is much influenced by the modulation or varying forms of the open passages, before it is articulated in the mouth; whilst with each motion of the tongue or lips there is a correspondence in the action of the velum and pharynx: so that the compression of the thorax, the adjustment of the larynx and glottis, the motions of the tongue and lips, and the actions of the pharynx and palate, must all consent before a word be uttered!

There is one part of the subject which I have omitted in the body of the paper. In speaking, the play of the chest is not the same as in the common act of breathing: the diaphragm is used less, and the ribs a great deal more. A man, preparing to speak, elevates his chest, whilst the abdomen is drawn flatter; the effect of which is to give more play to the elastic cartilages of the ribs, and the falling of the elevated chest is easy and unembarrassed; whereas, to expel the breath beyond a certain degree, requires the action of the muscles of expiration, and makes the act of speaking still more complicated.

When we think of the number of parts which must combine in office to produce the simplest articulate sound, we see the necessity for a corresponding intricacy of nervous connexions, and are less surprised to find the voice defective through derangement of the nervous system. In a person who stutters, the imperfection is obviously in the power of combination, not in the defect of any single part. Whilst he cannot combine the murmur from the glottis with the action of the pharynx, he can speak in a whisper; that is, he can articulate the faint sound of aspiration, whilst he cannot at the same time vocalize the breath. So he can sing his words without hesitation, or impediment, or spasm; because, in singing, the adjustment of the glottis and the due propulsion of the breath by the elevated chest, are accomplished and continue uninterruptedly. Neither does he experience any distress in pronouncing the vowels and liquid consonants, for the same reason: and if he study to commence his speech with a vowel sound, he can generally add to the vibration already begun, the proper action of the pharynx. Another necessary combination distresses a person who stutters, I mean the actions of the expiratory muscles and those of the throat. He expels the breath so much in his attempt at utterance, that to produce a sound at all, the ribs must be forcibly compressed. To remove this necessity, if he be made to fill his lungs and elevate the shoulders, the elasticity of the compages of the chest will come into play so as to expel the breath without effort, and he will speak with comparative facility and comfort. Accordingly, to commence speaking with the chest fully inflated, to pitch the voice properly, to keep a measured time in speaking, and to raise the voice on a liquid letter or vowel, are some of the common means recommended for the cure of stuttering; and they are certainly those which tend to overcome the difficulty in combining the organs of speech when the defect arises from no disorder or malformation of these organs taken separately.

I have only further to hope that, by the interest which this subject is capable of exciting, I may be indulged in a subsequent attempt to unravel the nerves of the neck and throat.

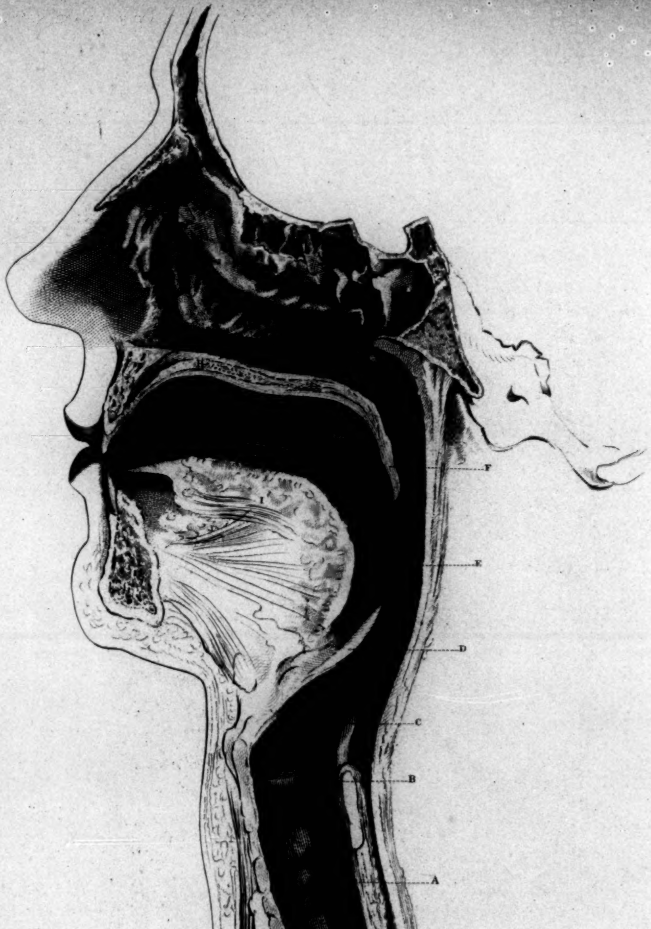

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Fig. 2.

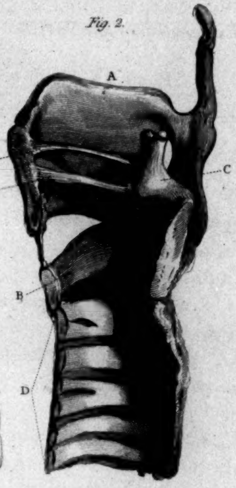


Fig. 1.

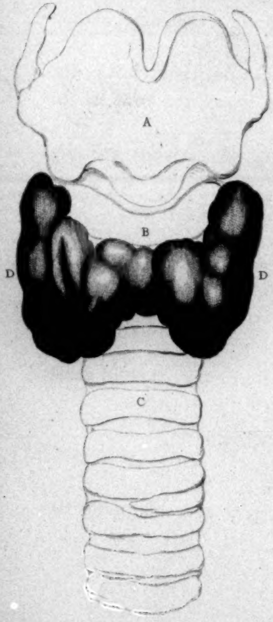
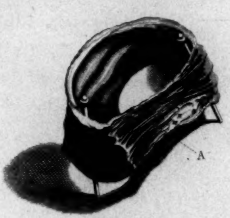


Fig. 3.



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Explanation of the PLATES.

PLATE IX.

This figure represents a section of the face and throat, exhibiting the organs of the voice in one view.

A. The trachea.

B. The chorda vocalis of the right side: above it we see the sacculus laryngis.

C. The arytenoid cartilage, which being moved by many muscles, changes the condition of the ligament or chorda vocalis.

D. The epiglottis, which falls like a valve over the glottis, as the morsel passes in swallowing, but which is important to the voice as directing the stream of vibrating air upon the fauces.

E. The bag of the pharynx, that cavity into which the sound is directed, and by the contraction of which an appulse is given in articulating certain consonants.

F. The uvula and velum palati, which, acting like a valve, and closing the passage upwards into the cavities of the face, throw the force of the contracting pharynx forwards into the mouth.

G. The cells of the bones of the face, through which some sounds are produced by reverberation.

H. The palate, the roof of the mouth, and floor of the nasal cavities.

I. The tongue.

All the dark or shaded part of the figure marks the extent of the cavities employed in the formation of the voice.

PLATE X.

Fig. 1. The larynx and trachea seen in front—in outline. The thyroid gland is shaded.

A. The thyroid cartilage.

B. The cricoid cartilage.

C. The trachea.

DD. The thyroid gland seated below the larynx and embracing the upper part of the trachea.

Fig. 2. Represents a section of the larynx and part of the trachea.

A. The thyroid cartilage.
B. The cricoid cartilage.
C. The arytenoid cartilage: on the top of it we see the surface for the articulation of the appendix.
D. The cartilaginous rings of the trachea.
E. The superior thyro-arytenoid ligament extending from the thyroid to the arytenoid cartilage.
F. The lower thyro-arytenoid ligament or chorda vocalis. Between these ligaments is formed the sacculus laryngis.

We perceive how the numerous muscles attached to the arytenoid cartilage, eight in number, must affect the ligament and alter the chink of the glottis.

Fig. 3. A portion of the trachea cut out to show the transverse muscle.

A. The transverse muscle.

XV. *Theory of the inverse Ratio which subsists between the Respiration and Irritability, in the Animal Kingdom. By* MARSHALL HALL, *M.D. F.R.S.E. M.R.I. &c. &c. Communicated by* J. G. CHILDREN, *Esq. Sec. R.S.*

Read February 23, 1832.

THE object of the investigation, of which the present paper details the principles, is to trace a peculiar law of the animal economy, through the various series, forms and conditions of animated being. This law may be announced in the following terms:

The quantity of the Respiration is inversely as the degree of the Irritability of the muscular fibre.

It will be necessary, in the very first place, to define the terms which I am about to employ. The expression inverse ratio is not used in its strict mathematical sense, but merely to designate the general fact, that, in cases in which the quantity of respiration is great, the degree of irritability is low; and that in cases in which the quantity of respiration is small, the degree of irritability is high. By the quantity of respiration, I mean the quantity of oxygen gas consumed, or exchanged for carbonic acid, in a given time, by the animal confined in atmospheric air. I have used the term irritability in the sense in which it is employed by GLISSON and HALLER,—to designate that peculiar property of the muscular fibre by which it contracts on the application of an appropriate stimulus; and I consider that muscle the most irritable which, cæteris paribus, contracts most and longest upon the application of the least degree of such stimulus. HALLER's definition of the term is very similar *. It must be confessed that the word irritability only expresses one half of the property or function of the muscular fibre,—its susceptibility to the influence of irritants or stimuli; the term contractility is equally defective,—expressing only the other half of that function, viz. the effect of that susceptibility under the actual influence of stimuli. The designation irrito-contractility would express the whole phenomena.

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* Mémoires sur la Nature sensible et irritable des Parties du Corps animal. Tome i. pp. 7—8, 75.

Organic life appears to result from the impression of stimuli upon parts endued with irritability. The principal stimuli in nature, are air, food, and heat; the principal and corresponding organs of irritability are the heart, the stomach, and the muscular system in general.

The animal series consists of beings variously modified by the varied degree of irritability, and by the varied quantity of stimulus. Throughout the whole these observe an inverse ratio. The bird tribes and the mammalia are characterized by great respiration, whilst the irritability of the muscular fibre is low; the reptiles, the batrachia and the fish tribes, on the other hand, are endued with a high degree of irritability, and little respiration. The higher parts of the zoological series consist of animals chiefly characterized by the appropriation of a great quantity of stimulus; the lower, by the high degree of irritability of the muscular fibre. The former are animals of stimulus—of activity; the latter are animals of irritability.

The due actions of life, in any part of the zoological series, appear to depend upon the due ratio between the quantity of atmospheric change induced by the respiration, and the degree of irritability of the heart: if either be unduly augmented, a destructive state of the functions is induced; if either be unduly diminished, the vital functions languish and eventually cease. If the bird possessed the degree of irritability of the reptile tribes, or the latter the quantity of respiration of the former, the animal frame would soon wear out. If, on the contrary, the bird were reduced to the quantity of respiration appro priate to the reptile, or the latter to the degree of irritability which obtains in the former, the functions of life would speedily become extinct. Various deviations from the usual proportion between the respiration and the irritability, however, occur, but there is an immediate tendency to restore that proportion; increased stimulus exhausts or lowers the degree of irritability, whilst diminished stimulus allows of its augmentation. The alternations between activity and sleep afford illustrations of these facts.

Changes in anatomical form in the animal kingdom present other illustrations of the law of the inverse proportion of the respiration and irritability.

The egg, the fœtus, the tadpole, the larva, &c. are respectively animals of lower respiration, and of higher irritability, than the same animals in their mature and perfect state. Changes in physiological condition also illustrate the same law. The conditions of lethargy, and of torpor, present examples of lower respiration, and of higher irritability, than the state of activity.

It may be remarked that whilst changes in anatomical form are always from lower to higher conditions of existence, changes in the physiological condition are invariably from higher to lower.

These views are further illustrated by a reference to the quantity of stimulus and the degree of irritability of each of the parts and organs of the animal system. But it is to the quantity of respiration, and the degree of irritability of the heart, that our attention is to be principally directed at this time. The oxygen of the atmospheric air is the more immediate and essential stimulus of this organ. Taken up in respiration, it is brought into contact with the heart, by means of the blood, which may be considered as the carrier of this stimulus, as it is of temperature and nutriment, to the various parts of the system. As oxygen is the principal stimulus, the heart is the principal organ of irritability, in all the vertebrated animals; if the contact of oxygen be interrupted, all perish in a greater or less period of time.

The extraordinary differences which exist in animals which occupy different stations in the zoological scale, have long excited the attention of naturalists. Nor have the differences which obtain in the various ages and states of its existence, in the same animal, escaped the attention of the physiologist. A similar remark applies to that singular state of existence and of the functions of life, designated hybernation. But it appears to me that a sufficiently comprehensive view has not been taken of the subject, and that many facts, with their multitudinous relations, still require to be determined.

I. *Of the Pneumatometer.*

The principal of these facts is that of the quantity of respiration. This is greater in proportion as the animal occupies a higher station in the zoological scale, being, among the vertebrated animals, greatest of all in birds, and lowest in fishes; the mammalia, the reptiles, and the amphibia occupy intermediate stations. The quantity of respiration is also remarkably low in the very young of certain birds which are hatched without feathers, and of certain animals which are born blind; and in hybernation it is almost extinct.

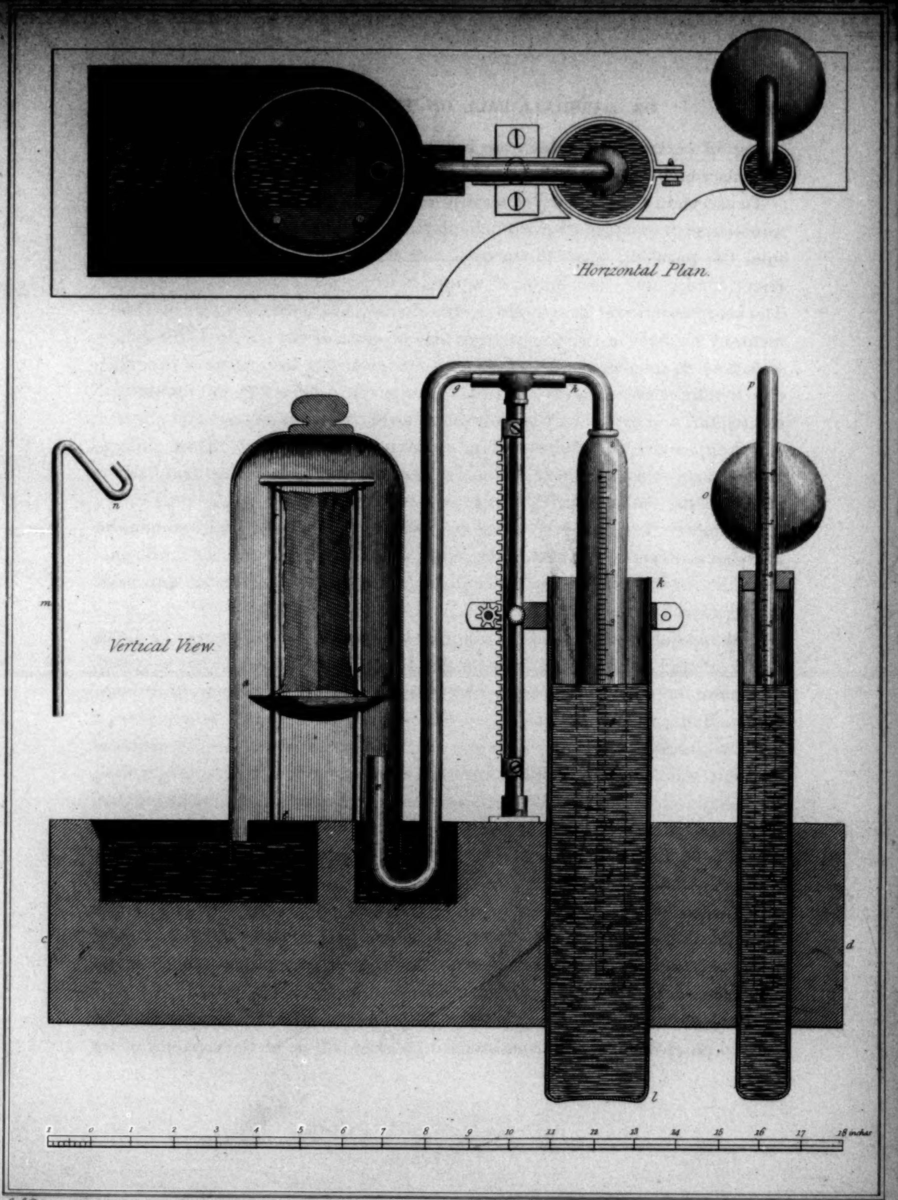
To ascertain the quantity of respiration in any given animal, with extreme minuteness, was a task of great difficulty. It was still more difficult to determine this problem, so as to represent the quantities of respiration in the different kinds, ages, and states of animals, in an accurate series of numbers. The changes induced in a given volume of air made the subject of experiment, by changes in the temperature and pressure of the atmosphere, and by variations in the height of the fluid of a pneumatic trough, which it is so difficult to appreciate minutely; the similar changes induced by the humidity of expired air, and by the heat of the animal itself, were so many and complicated, that it appeared almost impossible to arrive at a precise result. These difficulties, in fine, were such as to lead one of the first chemists of the present day to give up some similar inquiries in despair.

Fortunately I have been enabled to devise an apparatus which reduces this complex problem to the utmost degree of simplicity. I now beg the indulgence of the Society whilst I give a detailed description of its construction and mode of operation.

This apparatus, which I shall designate the *Pneumatometer*, consists of a glass jar *a b* (Plate XI.) inverted in a mercurial trough *c d*, so grooved and excavated, as accurately to receive the lower rim of the jar and the lowest part of the tube *e f g*, and also to admit of the animal which is made the subject of experiment, being withdrawn through the mercury. This jar communicates, by means of the bent tube *e f g h*, with the gauge *i j*, which is inserted into a larger tube, *k l*, containing water. A free communication between the jar and the external air is effected and cut off, at any time, by introducing and withdrawing the little bent tube *m n*, placing the finger upon the extremity *m*, whilst the extremity *n* is passed through the mercury.

If the jar be of the capacity of one hundred cubic inches, the gauge is to contain ten, and to be graduated into cubic inches and tenths of a cubic inch; so that each smallest division shall be the thousandth part of the whole contents of the jar.

Attached to the same mercurial trough is placed a little apparatus, *o p*, termed an *Aërometer*, and consisting of a glass ball *o*, of the capacity of ten



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cubic inches, communicating with a tube *p q*, bent at its upper part, of the capacity of one cubic inch, divided into tenths and hundredths, and inserted into a wider tube containing water, precisely in the manner of the gauge *i j*. In order to secure the exact proportion between the capacity of the pneumatometer and that of the aërometer, it is only necessary to add more or less of mercury to the trough.

The whole apparatus is inclosed in a glazed frame so as entirely to obviate the influence of partial currents of air. It is plain that changes in external temperature and pressure will affect both these parts of the apparatus equally; and that the fluids in the gauge *i j*, and in the tube *p q*, will move pari passu. It is therefore only necessary to compare them, and to take the difference, for the real alteration in the quantity of the gas in the jar.

Previously to noticing this difference, the fluids in the outer and inner tubes are to be brought accurately to the same level, by raising or depressing the outer tube *k l*, and the inner one *p q*.

In order that the air within the jar and that in the aërometer may be in the same state of humidity, a little water is introduced into the ball *o* of the latter.

When the animal is to be removed, the fluid in the inner and outer tubes of the gauge are to be brought to a precise level; the animal is then to be withdrawn through the mercury, by a cord attached to the little net or box in which it is secured; a quantity of fluid will immediately rise in the inner tube, *i j*, equal to the bulk of the animal; the bent tube, *m n*, is now to be passed through the mercury into the jar so as to effect a communication with the atmospheric air; a portion of air equal to the bulk of the animal rushes into the jar, whilst the fluids in the gauge regain their level.

To avoid the error which would arise from the influence of the temperature of the animal upon the air within the jar of the pneumatometer, the first observation of the degree upon the gauge must be made the instant the experiment is begun, and before the temperature of the animal can have been communicated to it; and the last, so long after the animal has been withdrawn as to allow of its restoration to the temperature of the atmosphere.

In this way all calculations for the varied temperature and pressure of the external air, for augmented humidity and temperature of the air of the pneumatometer, and for the changes in the height of the fluid of the trough, are at once disposed of in a manner the most accurate and simple.

It now remains to determine the quantity of change induced upon the air of the pneumatometer, by the respiration of the animal. Two views may be taken of this change; that of Messrs. Allen and Pepys, that the oxygen which disappears is replaced by a precisely equal bulk of carbonic acid; or that of M. Edwards, that there is generally an excess of the oxygen which disappears over that of the carbonic acid evolved. In either case the quantity of respiration is ascertained by the gauge of the pneumatometer in the following manner. A frame made of glass rods, *r s*, is placed within the jar *a b*, suspending portions of calico, imbued with a strong solution of pure potassa, and provided with a small dish of wood, so as to prevent the caustic liquid from dropping upon the animal beneath. By this means the carbonic acid is removed as it is evolved, or after the animal is withdrawn. The rise of the fluid in the gauge of the pneumatometer gives the quantity of oxygen which disappears,—whether this be entirely exchanged for carbonic acid, or only partly exchanged for carbonic acid, and partly absorbed,—and denotes the precise quantity of the respiration.

The question itself, of the entire or partial exchange of the oxygen gas which disappears, for carbonic acid gas evolved, is at once determined by employing the same apparatus without the solution of potassa: in the entire exchange, there is no alteration in the bulk of the air of the pneumatometer; in the case of a partial exchange, the alteration in the bulk of the air gives the precise excess of oxygen gas which disappears, over the quantity of carbonic acid evolved.

But this question, and that of the absorption and evolution of nitrogen, with the influence of night and day, of season, &c. are reserved for a future stage of this inquiry.

It is important that the animal should be left for a considerable time in the very situation in which it is to remain during the experiment, before that experiment is begun, and before the jar is placed over it. In this manner the effect of timidity or restlessness is allowed to subside, and prevented from mingling with that of the natural state of the respiration. A bit of cork must also be attached to the mercurial trough, so as to float upon the mercury at *t*, and prevent the disturbing effect of the contact of this fluid with the animal.

It is also well, after having placed the jar in the groove of the mercurial trough, to pour a little water over the mercury exterior to the jar. The apparatus is thus rendered perfectly air-tight, which is not always effected by the mercury alone.

By means of this apparatus we readily and accurately determine the quantity of the respiration of any given animal, in any given circumstances.

II. *Of the Measure of the Irritability.*

The problem to be next determined is that of the degree of irritability of the muscular fibre, and especially of the heart. This question is beset with scarcely fewer or less difficulties than that of the quantity of respiration, whilst it involves far greater errors and more discrepancy of opinion on the part of physiologists.

Even Baron Cuvier* has fallen into these errors. It will be shortly demonstrated that the degree of irritability is, in every instance, inversely as the quantity of respiration. Yet M. Cuvier, in a remarkable paragraph, states the very contrary, and even speaks of that which is the exhauster, as the repairer, of the irritability; whilst, on the other hand, he makes statements which appear to me at variance with this very opinion. In the Anatomie Comparée (tome i. p. 49), this celebrated writer observes, "Les expériences modernes ont montré qu'un des principaux usages de la respiration est de ranimer la force musculaire, en rendant à la fibre son irritabilité epuisée." See also tome iv. p. 301. Similar observations are made in M. Cuvier's more recent work, the Règne Animal: "C'est de la respiration que les fibres musculaires tirent l'énergie de leur irritabilité." tome i. p. 57. 2^{me} edit. "C'est la respiration qui donne au sang sa chaleur, et à la fibre la susceptibilité pour l'irritation nerveuse." tome ii. p. 1. On the other hand, speaking of the mollusca, (tome iii. p. 3.) M. Cuvier observes of those animals of low respiration, "L'irritabilité est extrême dans la plupart." The same term is, in fact, used in two distinct senses, in these paragraphs.

No further proof can be necessary of the extreme vagueness and incorrectness of the prevailing notions and expressions of physiologists in regard to this subject. All this will appear still more extraordinary, when the law, that the quantity of respiration and the degree of the irritability are, in fact, inverse throughout all the series, stages, and states of animated being, is clearly established.

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* Since this paper was read, science has experienced an irreparable loss in the death of this great man. I will not imagine that my comments upon what I conceive to be an error in his writings will be misinterpreted. No one can look upon Cuvier's labours with more sincere admiration than myself.

It is well known that the irritability of the heart and of the muscular fibre in general, is greater in the mammalia than in birds, and in reptiles and the amphibia than in the mammalia, whether we judge of it by the force and duration of the beat of the heart, exposed to the stimulus of the atmospheric air, or by the contractions of the other parts of the muscular system. Now this is precisely the order of the quantity of respiration in these animals, as ascertained by the pneumatometer, inverted. It is essential, in accurately determining the question of the irritability of the muscular fibre, to compare animals of the same class inter se; birds and the mammalia, reptiles and the amphibia, fishes, the mollusca, &c. must be compared with each other, both generically and specifically. It is especially necessary to compare the warm-blooded, the cold-blooded, the air-breathers, and the water-breathers, in this manner. However the different classes may differ from each other, there are differences in some of the species of the same class, and especially that of fishes, scarcely less remarkable.

Great differences in the duration of the beat of the heart, are observed in the fœtal, early, and adult states of the higher animals; this duration being greatest in the first, and least in the last of these conditions. The order of the quantity of respiration is inverse.

The law of the irritability being inversely as the respiration, obtains even in the two sides of the heart itself, in the higher classes of animals. The beat of the heart removed from the body, does not cease at the same time in the walls of all its cavities, or of its two sides: but, as HARVEY observes, "primus desinit pulsare sinister ventriculus; deinde ejus auricula; demum dexter ventriculus; ultimo (quod etiam notavit GALENUS) reliquis omnibus cessantibus et mortuis, pulsat usque dextra auricula*."

Even in this case the irritability is greatest in the part in which the respiration is least.

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* Opera Omnia, Collegio Medicorum Londinensi edita, 1766, p. 28.

It was shown by **Hook**, in the early days of the Royal Society*, that if, the respiration being suspended, an animal appeared to be dying, the beat of the heart and the signs of life were speedily restored, on performing artificial respiration, or even by forcing air through the trachea, bronchia, and pulmonary air-cells and allowing it to escape through incisions made through the pleura.

It was, in the next place, clearly shown by **Goodwyn**, in one of the most beautiful specimens of physiological inquiry in any language†, that in suspended respiration, it is the left side of the heart which first ceases to contract, the right side still continuing its function for several minutes, until the supply of blood may be supposed to fail.

The facts detailed by **Harvey** had shown that the left side of the heart was endued with less irritability than the right; the experiment of **Hook**, that respiration restored the action of the heart, if it had previously ceased; that of **Goodwyn**, that this cessation and restoration of functions were observed in the left side of the heart. It was obvious, on the other hand, that the respiration belongs, as it were, to the left side of the heart.

It appears plainly deducible from these facts, that in circumstances and structures the most similar, the respiration is accurately inversely as the irritability.

For the sake of a comparison with the hybernating animal, the object of which will be explained hereafter, I thought it right to repeat this experiment.

Before I proceed to detail the result, I may just describe an easy method of performing that part of it which consists of artificial respiration. A quill is firmly fixed in the divided trachea; a small hole is then cut into that part of the quill which is external; **Read's** syringe is then adapted to the other end of the quill. At each motion of the piston downwards, the lungs are distended; whilst the piston is raised, the air escapes through the opening in the quill, producing expiration. The experiment, therefore, only requires the common action of the syringe.

The experiment itself answered my expectation. During the cessation of respiration, the left ventricle ceased to beat, the right ventricle retaining its function; on renewing the respiration, the left ventricle resumed its beat. It appears from this experiment, that from want of a degree of irritability equal to that of the right ventricle, and its own proper stimulus of arterial blood, the left ventricle ceased its contractions. The function of the right ventricle must soon cease in consequence, from want of a supply of blood.

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* Phil. Trans. vol. ii.

† On the Connexion of Life with Respiration: London, 1788, pp. 72, 82 note.

These facts prove that arterial blood is the necessary stimulus of the left side of the heart, its irritability being low; but that venous blood is a sufficient stimulus of the right, from its higher irritability: the phenomena plainly flow from the law, that the quantity of respiration and the degree of irritability, observe an inverse ratio to each other, and from the facts on which that law is founded. In this double sense, besides that of distinct cavities, the mammalia have, therefore, two hearts; and as the highly aerated blood of the left is the peculiar property of birds and the mammalia, so the highly irritable fibre of the right may be compared to that of the heart of reptiles and the fishes.

Except for the objection to new terms, the left side of the heart might be termed arterio-contractile, and the right veno-contractile; the first being stimulated by arterial, the second by venous blood.

It is quite obvious that the heart will bear a suspended respiration better, the more nearly its irritability approaches to that which may be designated veno-contractile. *The power of bearing a suspended respiration thus becomes a measure of the irritability.* It is expressed, numerically indeed, by the length of time during which the animal can support a suspended respiration; a conclusion of the highest degree of importance in the present inquiry.

Birds die almost instantly on being submerged in water; the mammalia survive about three minutes, the reptiles and the batrachia a much greater length of time.

The unborn fœtus, the young animal born with the foramen ovale open, the reptile, the mollusca, having all a state of the heart approaching to the veno-contractile, bear a long-continued suspension of the respiration, compared with the mature animal of the higher classes.

But the most remarkable fact deducible from this reasoning is the following: if such a case existed as that of the left side of the heart being nearly or absolutely veno-contractile, such an animal would bear the indefinite suspension of respiration such an animal would not drown though immersed in water. Now there is precisely such a case. It is that of the hybernating animal. It will be shown in the subsequent paper, that in the state of perfect hybernation the respiration is nearly suspended; the blood must, therefore, be venous. Yet the heart continues to contract, although with a reptile slowness. The left ventricle is, therefore, veno-contractile, and in this sense, in fact, sub-reptile. The case forms a sole exception to the law pointed out by HARVEY, that the left ventricle ceases to contract sooner than the right. If in the hybernating animal the left ventricle does cease to beat sooner than the right, it is only in so slight a degree as to be referred to the greater thickness of its parietes, and the slight degree in which respiration still remains. It is obvious that the foregoing statement must be taken with its due limitations. Venous blood is unfit for the other animal purposes, even though it should stimulate the heart to contraction.

Another mode of determining the degree of irritability, is the application of stimuli, as galvanism. A muscular fibre endued with high irritability, as that of the frog, and the galvanic agency are mutually tests of each other*.

A third criterion and measure of the irritability is afforded by the influence of water at temperatures more or less elevated, in inducing permanent contraction of the muscular fibre.

There are two other properties of animals which depend upon the varied forms of the inverse ratio which exists between the respiration and the irritability. The first is *activity*, the second, *tenacity of life*.

The activity, which, I believe, M. CUVIER has confounded with the irritability, is generally directly proportionate to the respiration, and intimately depends upon the condition of the nervous system resulting from the impression of a highly arterial blood upon its masses, and not upon the degree of irritability of the muscular fibre. It is the pure effect of high stimulus.

To show that M. CUVIER has blended the idea of the irritability of the muscular fibre with that of the activity of the animal, it is only necessary to recur to the passages already quoted from that author, and to adduce the observations with which they are connected. "On vient de voir à quel point les animaux vertébrés se ressemblent entre eux; ils offrent cependant quatre grandes subdivisions ou classes, caractérisées par l'espèce ou la force de leurs mouvements, qui dépendent elles-mêmes de la quantité de leur respiration, attendu que c'est de la respiration que les fibres musculaires tirent l'énergie de leur irritabilité *." "Comme c'est la respiration qui donne au sang sa chaleur, et à la fibre la susceptibilité pour l'irritation nerveuse, les reptiles ont le sang froid, et les forces musculaires moindres en totalité que les quadrupèdes, et à plus forte raison que les oiseaux; aussi n'exercent-ils guère que les mouvements du ramper et du nager; et, quoique plusieurs sautent et courent fort vite en certains moments, leurs habitudes sont généralement paresseuses, leur digestion excessivement lente, leurs sensations obtuses, et dans les pays froids ou tempérés, ils passent presque tous l'hiver en léthargie †."

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* BOSTOCK on Galvanism, pp. 4, 14.

It is extraordinary that M. CUVIER should have associated the elevated temperature of the blood with a high irritability of the muscular fibre, when they are uniformly separated in nature, and are, indeed, absolutely incompatible in themselves. The muscular fibre of the frog is so irritable, that it would instantly pass into a state of rigid and permanent contraction, if bathed with a fluid of the temperature of the blood of birds‡.

The same confusion of ideas on the subject of the activity of the animal and the irritability of the muscular fibre prevails, I believe, amongst our own physiologists; at least, in conversation with two, who may rank amongst the first, I found that they had uniformly considered the respiration and the irritability to be directly, instead of inversely, proportionate to each other.

That singular and interesting property of the lower orders of animals termed tenacity of life is, on the other hand, distinctly associated with a high degree of irritability of the muscular fibre. This property may be defined as consisting of the power of sustaining the privation of respiration, the privation of food, various mutilations, divisions, &c. It is greater as we descend in the zoological scale. As activity depends upon the presence and condition of the spino-cerebral masses acted upon by arterial blood, tenacity of life depends upon the diminution or absence of these masses and of this highly arterialized blood,

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* Le Règne Animal, tome i. pp. 56, 57. 2^{me} edit.

† Ibid. tome ii. pp. 1, 2. 2^{me} edit.

‡ See An Essay on the Circulation, chap. vii. pp. 180, 181.

being greatest of all in those animals which approach a mere muscular structure. Almost the sole vital property then remaining is the irritability; and this property does not immediately suffer from division.

It is possible to reduce some of the reptile tribes to a state approaching that of animals still lower in the scale, by removing, by very slow degrees, successive portions of the nervous masses. This is most readily done in animals in which the respiration is already low, and the irritability high, as in the fœtus, in the very young animal, in the reptile, &c., as in the experiments of LEGALLOIS*, M. SERRES†, myself‡, &c.

There is, even in animals most tenacious of life, one kind of mutilation—one kind of injury not well borne. As the blood is in its lowest condition of stimulus, it cannot be withdrawn with impunity; frogs even soon perish if their blood be allowed to flow. As the irritability, on the other hand, is high, certain stimuli, as galvanism, slightly elevated temperatures, &c. are speedily fatal. The batrachia are promptly destroyed by immersion in water of a temperature of 108° of FAHR., and some fish and crustacea perish in great numbers under the influence of a thunder-storm. It is a singular fact, that the fish alone, whose food is found amongst animals of a high irritability, should possess an electrical organ for the destruction of its prey.

Having stated the law of the inverse ratio of the quantity of respiration, and of the degree of irritability of the muscular fibre, especially in the heart, I purpose to trace it, by a series of observations, through the zoological scale, and in the different stages and states of animal existence. This inquiry will be followed by an investigation into the quantity of respiration, in different temperatures and seasons, in animals which retain, and animals which lose their temperature; it is obvious, à priori, that the former must have a lower respiration in the elevated temperatures of summer than in winter, whilst the irritability, and with it the power of supporting the privation of air, will observe an inverse ratio; in the latter, it is probable that other laws prevail.

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* Experiences sûr le Principe de la Vie.

† Anatomie Comparée du Cerveau, tome ii. p. 224.

‡ Essay on the Circulation, chap. iii. § 1.

A particular object which I have in view is to construct accurate Tables of the quantity of respiration and the degree of irritability, which cannot fail to have many important applications in physiology. They will especially afford many explanations of the facts detailed in the extraordinary works of LEGALLOIS and M. EDWARDS, as I shall have occasion to point out particularly hereafter. The facts in regard to the irritability, ascertained by NYSTEN* and MANGILI†, insulated and useless hitherto, will assume a new and high degree of importance. The law of the inverse ratio which subsists in the animal kingdom between the respiration and the irritability of the muscular fibre, which admits of being extended so as to include all stimuli, appears to me, indeed, to constitute a chain which links together all the phenomena of the animal economy. I believe it to be the most general and inclusive in physiology.

* Recherches de Physiologie, sect. iv.

† Annales du Museum, tome x. p. 434.

XVI. *On Hybernation.* *By* Marshall Hall, *M.D. F.R.S.E. M.R.I. &c. &c.*

Communicated by J. G. Children, *Esq. Sec. R.S.*

Read March 1st and 8th, 1832.

That peculiar condition of certain mammalia during the winter season, which has been designated hybernation, has been aptly compared by various authors to ordinary sleep. In both the respiration is diminished. This fact was first determined, in regard to sleep, by Messrs. Allen and Pepys[*]. It obtains in a much higher degree in the state of hybernation. It is highly probable that in sleep, as in hybernation, the irritability of the muscular fibre becomes augmented. These two conditions of the animal system may therefore mutually illustrate each other.

Ordinary sleep is similar to the sleep of the hybernating animal; and the sleep of the hybernating animal is similar to that deeper sleep, or lethargy, which is designated hybernation. We are thus led to trace a connexion between the recurrent sleep of all animals, and the deep and protracted sleep of a few.

I. *Of the Sleep of hybernating Animals.*

In the sleep of the hybernating animal, the respiration is more or less impaired: if the animal be placed in circumstances which best admit of observation, the acts of respiration will be found to have greatly diminished; if it be placed in the pneumatometer, little alteration is induced in the bulk of the air; if its temperature be taken by the thermometer, it will be found to be many degrees lower than that of the animal in its active state; if it be deprived of atmospheric air, it is not immediately incommoded or injured.

These facts I have observed in the hedge-hog[†], the dormouse[‡], and the bat[§]. If other authors have not made the same observations, it is because they have not been aware how easily this sleep is disturbed. To walk over the floor, to touch the table, is sufficient, in many instances, to rouse the animal, to re-produce respiration, and to frustrate the experiment.

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[*] Phil. Trans. for 1809.

[†] Erinaceus Europæus.

[‡] Myoxus avellanarius.

[§] Vespertilio noctula.

The bat, which is a crepuscular or nocturnal feeder, regularly passes from its state of activity to one which may be designated diurnation. The respiration and the temperature fail; the necessity for respiration is greatly lessened.

During the summer of 1831, I carefully observed a bat in this condition. If it were quite quiet, its respiration became very imperfect; its temperature was but a few degrees above that of the atmosphere; being placed under water, it remained during eleven minutes uninjured, and on being removed became lively and continued well.

I have more recently watched the habits of two hedgehogs, in a temperature varying from 45° to 50°. These animals alternately awake, take food, and fall asleep. One of them is frequently awake, whilst the other is dormant, and goes to sleep at a time that the other awakes, but without regularity. When awake, the temperature of each, taken by pressing the bulb of a thermometer upon the stomach, is about 95°; when dormant, it is 45°; that of the atmosphere being 42° or 43°. The duration of this sleep is from two to three days, according to the temperature of the atmosphere. On the 4th of February, 1832, the temperature of the atmosphere being 50°, both the hedgehogs were dormant,—the temperature of one was 51°, and that of the other 52°; on the succeeding day, the temperature of the atmosphere had fallen one degree, the temperature of one of the hedgehogs was 49°, whilst that of the other, which had become lively, had risen to 87°; on the succeeding day, the first had become somewhat lively, and its temperature had risen to 60°, that of the other being 85°, and that of the atmosphere 47°.

I have observed precisely the same alternations in the dormouse; except that this animal awakes daily in moderate temperatures, takes its food, and passes into a state of sleep, in which the respiration is greatly impeded, and the temperature little higher than that of the atmosphere.

On the day on which the observations were made on the hedgehogs, the atmosphere being 49°, that of two dormice was 52°; on the succeeding day, the external temperature being 47°, that is, lower by two degrees, the temperature of one of these dormice was 92°, and that of the other 94°; and only three hours afterwards, the temperatures were 60° and 70° respectively, with a slight appearance of lethargy.

The hedgehog and the dormouse appear, in fact, to awake from the call of hunger, then to eat, and then again to become dormant, in temperatures which may be termed moderate. The bat, which could not find food if it did awake, does not undergo these periodical changes, except in the summer season. It appears to me, from the most careful observation, that there is every degree between the ordinary sleep of these animals and the most profound hyber nation.

It is quite obvious, from these observations, that the ordinary sleep of hybernating animals differs from that of others, by inducing a more impaired state of the respiration and of the evolution of heat, with an augmented power of bearing the abstraction of the atmospheric air. This sleep probably passes into true hybernation, as the blood which circulates through the brain becomes more and more venous, from the diminution of the respiration, and as the muscular fibre of the heart acquires increased irritability.

It is absolutely necessary, in comparing the powers of hybernating and other animals, of evolving heat, accurately to observe whether there be any degree of sleep. Mr. Hunter's and M. Edwards's experiments are extremely deficient, for want of this attention. Mr. Hunter, comparing the common mouse and the dormouse exposed to a very low temperature, observes, that the heat of the former "was diminished 16° at the diaphragm, and 18° in the pelvis, while in the dormouse it gained five degrees, but lost upon a repetition." The explanation of these facts is afforded by noticing that when the dormouse increased in temperature, it was "very lively," but on the "repetition" it had become "less lively*." M. Edwards omits to mention whether the hybernating animals in his experiments were disposed to be lively or dormant, or whether they had recently recovered from a dormant state. Without a peculiar attention to this point, no correct result can be obtained. The hybernating animal in a state of vigour and activity, is a totally different being from the same animal disposed to become dormant.

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* Animal Œconomy, p. 114.

II. *Of true Hybernation.*

I now proceed to the detail of my observations upon actual hybernation, and especially upon the state of the respiration and the irritability, of the sensibility, the circulation, and the digestion, in this singular condition of the animal economy.

1. *Of the Respiration.*

The respiration is very nearly suspended in hybernation. That this function almost ceases, is proved, 1st, by the absence of all detectible respiratory acts; 2ndly, by the almost entire absence of any change in the air of the pneumatometer; 3rdly, by the subsidence of the temperature to that of the atmosphere; and 4thly, by the capability of supporting, for a great length of time, the entire privation of air.

1. I have adopted various methods to ascertain the entire absence of the acts of respiration. I placed bats in small boxes, divided by a partition of silk ribbon, the cover of which consisted of glass, and in the side of which a small hole was made to admit of placing a long light rod or feather under the animal's stomach. The least respiratory movement caused the extremity of this rod to pass through a considerable space, so that it became perfectly apparent.

Over the hybernating hedgehog I placed a similar rod, fixing one extremity near the animal, and leaving the other to move freely over an index. During hybernation not the slightest movements of these rods could be observed, although they were diligently watched. But the least touch, the slightest shake immediately caused the bat to commence the alternate acts of respiration, whilst it invariably produced the singular effect of a deep and sonorous inspiration in the hedgehog. It is only necessary to touch the latter animal to ascertain whether it be in a state of hybernation, or not: in the former case there is this deep sonorous inspiration; in the latter, the animal merely moves and coils itself up a little more closely than before. After the deep inspiration, there are a few feeble respirations, and then total quiescence. The bat makes similar respirations without the deep inspiration, and then relapses into suspended respiration.

2. As the acts of respiration are nearly suspended during hybernation, so are the changes induced in the atmospheric air.

On January the 28th, the temperature of the atmosphere being 42°, I placed a bat in the most perfect state of hybernation and undisturbed quiet, in the pneumatometer, during the whole night, a space of ten hours, from $1^h\ 30^m$ to $11^h\ 30^m$. There was no perceptible absorption of gas.

Having roused the animal a little, I replaced it in the pneumatometer, and continued to disturb it from time to time, by moving the apparatus. It continued inactive, and between the hours of $1^h\ 20^m$ and 4^h, there was the absorption of one cubic inch only of gas.

Being much roused at four o'clock, and replaced in the pneumatometer, the bat now continued moving about incessantly; in one hour, five cubic inches of gas had disappeared. It was then removed. A further absorption took place of ·8 of a cubic inch of gas.

Thus the same little animal, which, in a state of hybernation, passed ten hours without respiration, absorbed or converted 5·8 cubic inches of oxygen gas into carbonic acid, in one hour, when in a state of activity. In an intermediate condition, it removed one cubic inch of oxygen in two hours and forty minutes.

I repeated this experiment on February the 18th. A bat, in a state of perfect hybernation, was placed in the pneumatometer, and remained in it during the space of twenty-four hours. There was now the indication of a very slight absorption of gas, not, however, amounting to a cubic inch.

On February the 22nd, I repeated this experiment once more, continuing it during the space of sixty hours; the thermometer descended gradually, but irregularly, from 41° to 38°; the result is given in the subjoined Table.

Date.		External Temperature.	Absorption.	Duration. h
February 22	11 P.M. . .	41°		
23	11 A.M. . .	38½ . .	·8 . .	12
	11 P.M. . .	39½ . .	·75 . .	12
24	11 A.M. . .	38 . .	·5 . .	12
	11 P.M. . .	39 . .	·75 . .	12
25	11 A.M. . .	38 . .	·6 . .	12
			3·4	60

From this experiment it appears that 3·4 cubic inches of oxygen gas disappeared in sixty hours, from the respiration of a bat in the state of lethargy. It has been seen that in a state of activity, an equal quantity of this gas disappeared in less than half that number of minutes. The respiration of the hybernating bat descends to a sub-reptile state; it will be seen shortly that the irritability of the heart and of the muscular fibre generally, is proportionably augmented.

In this experiment it is probable that the lethargy of the animal was not quite complete. Should the temperature of the atmosphere fall, and continue at 32°, I shall again repeat it under these circumstances. The respiration will probably be still more nearly suspended.

It is important to remark, that the registration of the quantity of absorption in these experiments was not begun until several hours after the animal had been inclosed within the jar of the pneumatometer, so that the absorption of the carbonic acid always present in atmospheric air, was excluded from the result.

It may be a question whether the slight quantity of respiration I have mentioned be cutaneous. The absence of the acts of respiration would lead us to this opinion. But it may be observed, that these acts have not been watched, and can scarcely be watched continuously enough, to determine the question of their entire absence. Some contrivance to ascertain whether the rod has moved along the index during the absence of the observer, would resolve every doubt upon this interesting point. And I think it right to remark, that after the apparent total cessation of respiration, as observed by the means which have just been described, there is probably still a slight diaphragmatic breathing. I am led to this conclusion, by having observed a slight movement of the flank in a favourable light, unattended by any motion of the thorax or epigastrium.

3. Much precaution is required in ascertaining the comparative temperature of the animal with that of the atmosphere. The slightest excitement induces a degree of respiration, with the consequent evolution of heat.

The plan which is best adapted to determine this question in regard to the bat, and which I have adopted, together with every attention to preserve the animal quiet and undisturbed, is the following: A box was made of mahogany, with a glass lid, divided horizontally at its middle part, by a fold of strong ribbon, and of such dimensions as just to contain the animal. The bat was placed upon the ribbon, and inclosed by fixing the lid in its place. Being lethargic, it remained in undisturbed quiet. A thermometer, with a cylindrical bulb, was now passed through an orifice made in the box on a level with the ribbon, under the epigastrium of the animal, and left in this situation.

It was only now necessary to make daily observations and comparisons between this thermometer and another placed in the adjacent atmospheric air. The layer of silk, and the portion of air underneath, protected the animal from the immediate influence of the temperature of the table, on which the box was placed.

The following Table gives the result of observations made during many days, in very varying temperatures.

Date.			Temperature of the Atmosphere.	Temperature of the Animal.
January	6	11 P.M.	40°	40½°
	7	8 P.M.	43	43
	8		41	41½
	9	11 P.M.	47	46
	10	10 A.M.	46	46
	—	12 midnight	47	47
	11	10 P.M.	45	45
	12	11 P.M.	45	45
	13	11 P.M.	37	37½
	14	11 A.M.	37	37
	—	11 P.M.	40	40
	15	2 P.M.	37	37
	—	11 P.M.	35	35
	16	11 P.M.	37	37
	17	11 P.M.	42	42
	18	11 A.M.	40	40
	19	10 P.M.	36	36
	20	11 P.M.	39	39
	21	11 P.M.	40	40
	22	11 P.M.	44	44

Date.		Temperature of the Atmosphere.	Temperature of the Animal.
		°	°
January 23	10 A.M.	42½	42½
—	11 P.M.	40½	40½
24	11 P.M.	43½	43½
25	10 P.M.	42	42
26	10 P.M.	41	41
27	10 P.M.	37	37
28	11 A.M.	34½	34½
—	11 P.M.	37	37
29	11 A.M.	42	42
—	11 P.M.	43	43
30	11 P.M.	42	42
31	11 P.M.	39½	39½

From this Table it is obvious that the temperature of the hybernating animal accurately follows that of the atmosphere. When the changes of temperature in the latter are slight, the two thermometers denote the same temperature. If these changes are greater and more rapid, the temperature of the animal is a little lower or higher, according as the external temperature rises or falls; a little time being obviously required for the animal to attain that temperature.

Similar observations were made during the first three days of February. On the 4th, however, the temperature of the atmosphere rose to 50½°; that of the animal was now 82°, and there was considerable restlessness. On the 6th, the temperature of the atmosphere had fallen to 47½°, and that of the animal to 48°, whilst there was a return of the lethargy.

After this period there were the same equal alterations of temperature in the animal and in the atmosphere, observed in the month of January.

It is only necessary to add to these observations, that the internal temperature is about three degrees higher than that of the epigastrium. In two bats, the external temperature of each of which was 36°, a fine thermometer, with an extremely minute cylindrical bulb, passed gently into the stomach, rose to 39°.

The following experiments, made by the celebrated Jenner, illustrate this point:

"In the winter, the atmosphere at 44°, the heat of a torpid hedgehog at the pelvis was 45°, and at the diaphragm 48½°.

"The atmosphere 26°, the heat of a torpid hedgehog, in the cavity of the abdomen, was reduced so low as 30°.

"The same hedgehog was exposed to the cold atmosphere of 26° for two days, and the heat of the rectum was found to be 93°; the wound in the abdomen being so small that it would not admit the thermometer*.

"A comparative experiment was made with a puppy, the atmosphere at 50°; the heat in the pelvis, as also at the diaphragm, was 102°.

"In summer, the atmosphere at 78°, the heat of the hedgehog, in an active state in the cavity of the abdomen, towards the pelvis, was 95°; at the diaphragm, 97°†."

There is an error in the admirable work of M. Edwards, in relation to the present subject, which it is important to point out. M. Edwards first ascertained the interesting fact, that the very young of those species of animals which are born blind, lose their temperature if removed from the contact of their parent; and justly concludes that they have not sufficient power of evolving heat, to maintain their natural temperature when so exposed. M. Edwards then subjected hybernating animals to the action of cold, and observing that their temperature also fell, he concludes that they, like the very young animal, have not the faculty of maintaining their temperature under ordinary circumstances ‡.

It is remarkable that this acute physiologist did not perceive the error in this reasoning. In no instance does the young animal maintain its warmth, when exposed alone to the influence of an atmosphere of moderate temperature. Can this be said of the hybernating animal? Certainly not. In ordinary temperatures, the hybernating animal maintains its activity, and with its activity, its temperature. The loss of temperature in this kind of animal is an induced condition, occasioned by sleep. Nothing, therefore, can be more incorrect than the following conclusion: "Au mois d'Avril 1819, l'air étant à 16°, une chauve-souris adulte, de l'espèce nommée *oreillard*, avait une température de 34°. Elle était récemment prise et en bon état. Je la plaçai dans un vase de terre que je refroidis en l'entourant de glace pilée et d'un peu de sel. L'air y était à 1°. Un couvercle était placé de manière à établir une libre communication avec l'air extérieur. Après y avoir laissé la chauve-souris pendant une heure, sa température était réduite à 14°. Elle s'était donc refroidée de 20° dans un si court espace de temps, sous la seule influence d'une température qui n'était pas au-dessous de zéro. Des cochons d'Inde, des oiseaux adultes, placés dans les mêmes circonstances, ne se sont refroidis que de deux ou trois degrés au plus, quoiqu'on ait continué l'influence du froid pour compenser les différences de volume.

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* The animal had become lively. See Hunter on the Animal Œconomy, p. 113.

† Ibid. p. 112.

‡ Des Agens Physiques, p. 155.

"Nous voyons par là que les chauves-souris produisent habituellement moins de chaleur que ces animaux à sang chaud, et que c'est principalement à cette cause qu'il faut attribuer l'abaissement de leur température pendant la saison froide. En comparant cette expérience sur la chauve-souris adulte avec celles que nous avons faites sur les jeunes animaux à sang chaud, on y aperçoit un rapport remarquable; ils ne produisent pas assez de chaleur pour soutenir une température élevée, lorsque l'air est à un degré voisin de zéro. Mais il y a cette différence, que c'est un état passager chez les jeunes animaux à sang chaud, et qu'il est permanent chez les chauves-souris.

"Il est évident que les autres mammifères hibernans doivent participer plus ou moins de cette manière d'être. Les faits que j'ai exposés suffisent pour nous faire considérer ce groupe d'animaux sous le point de vue suivant; qu'au printemps et en été, dans leur état d'activité et de veille, lorsque leur température est assez élevée pour ne pas différer essentiellement de celle qui caractérise les animaux à sang chaud, ils n'ont pas la faculté de produire autant de chaleur *."

There is a point unnoticed in M. EDWARDS's experiment. It is the condition of the bat in regard to activity or lethargy under the exposure to cold; and upon this the whole phenomena depend.

The differences between the young animal benumbed, and the hybernating animal lethargic, from cold, are both great and numerous. I purpose to point them out particularly on a future occasion.

4. It is in strict accordance with these facts, that the lethargic animal is enabled to bear the total abstraction of atmospheric air or oxygen gas, for a considerable period of time.

SPALLANZANI placed a marmot in carbonic acid gas, and makes the follow-

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* Des Agens Physiques, p. 154.

ing report of the experiment in a letter to **Senebier**: "Vous vous ressouviendrez de ma marmotte qui fut si fortement léthargique dans l'hiver sévère de 1795; je la tins alors pendant quatre heures dans le gaz acide carbonique, le thermomètre marquant —12°, elle continua de vivre dans ce gaz qui est le plus mortel de tous, comme je vous le disais: au moins un rat et un oiseau que j'y plaçai avec elle y périrent à l'instant même. Il parait donc que sa respiration fut suspendue pendant tout ce tems-là. Je soumis à la même expérience des chauve-souris semblablement léthargiques, et le résultat fut le même*."

A bat which was lethargic in an atmosphere of 36° was immersed in water of 41°. It moved about a little, and expelled bubbles of air from its lungs. It was kept in the water during sixteen minutes, and then removed. It appeared to be uninjured by the experiment.

A hedgehog which had been so lethargic in an atmosphere of 40° as not to awake for food during several days, was immersed in water of 42°. It moved about and expelled air from its lungs. It was retained under the water during 22½ minutes. It was then removed. It appeared uninjured.

It seems probable that the motions observed in these animals were excited through the medium of the cutaneous nerves.

The power of supporting the abstraction of oxygen gas, or atmospheric air, belongs solely to the hybernating state, and is no property of the hybernating animal in its state of activity. After having found that the dormant bat, in summer, supported immersion in water, during eleven minutes, uninjured, I was anxious to know whether the active hedgehog possessed the same power. I immersed one of these animals in water. It expired in three minutes, the period in which immersion proves fatal to the other mammalia. Sir **Anthony Carlisle** has, therefore, committed an error, somewhat similar to that of M. **Edwards**, when he asserts that "animals of the class Mammalia, which hybernate and become torpid in winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit †." The power of bearing a suspended respiration is an induced state. It depends upon sleep or lethargy themselves, and their effect in impairing or suspending respiration; and upon the peculiar power of the left side of the heart, of becoming veno-contractile under these circumstances.

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* Mémoires sur la Respiration, par **Lazare Spallanzani**, traduits en Français, d'après son manuscrit inédit; par **Jean Senebier**: p. 75.

† Phil. Trans. 1805, p. 17.

2. *Of the Irritability.*

The single fact of a power of sustaining the privation of air, without loss of life, leads alone to the inference that the irritability is greatly augmented in the state of hybernation. This inference flows from the law so fully stated in my former paper, and the fact is one of its most remarkable illustrations and confirmations.

It might have been inferred from these premises, that the beat of the heart would continue longer after decapitation in the state of hybernation, than in the state of activity in the same animal; an inference at once most singular and correct.

This view receives the fullest confirmation from the following remarkable experiment: On March the 9th, soon after midnight, I took a hedgehog which had been in a state of uninterrupted lethargy during 150 hours, and divided the spinal marrow just below the occiput; I then removed the brain and destroyed the whole spinal marrow as gently as possible. The action of the heart continued vigorous during four hours, when, seeing no prospect of a termination to the experiment, I resolved to envelope the animal in a wet cloth, and leave it until early in the morning. At 7 o'clock A.M. the beat of both sides of the heart still continued. They still continued to move at 10 A.M., each auricle and each ventricle contracting quite distinctly. At half after 11 A.M. all were equally motionless; yet all equally contracted on being stimulated by the point of a penknife. At noon the two ventricles were alike unmoved on being irritated as before; but both auricles contracted. Both auricles and ventricles were shortly afterwards inirritable.

This experiment is the most extraordinary of those which have been performed upon the mammalia. It proves several interesting and important points: 1. That the irritability of the heart is augmented in continued lethargy in an extraordinary degree. 2. That the irritability of the left side of the heart is then little, if at all, less irritable than the right,—that it is, in fact, veno-contractile. 3. That, in this condition of the animal system, the action of the heart continues for a considerable period independently of the brain and spinal marrow.

On April the 20th, at six o'clock in the evening, the temperature of the atmosphere being 53°, a comparative experiment was made upon a hedgehog in its state of activity: the spinal marrow was simply divided at the occiput; the beat of the right ventricle continued upwards of two hours, that of the left ventricle ceased almost immediately; the left auricle ceased to beat in less than a quarter of an hour; the right auricle also ceased to beat long before the right ventricle.

In further proof of the same fact, I may here adduce a remarkable paragraph from the paper of Mangili in the Annales du Muséum*: "J'observai à peu près les mêmes choses dans une autre marmotte en léthargie, que je decapitai le 22 de Mars 1807. Mais en ouvrant celle-ci, j'avois deux objets: le premier, d'examiner l'état des viscères les plus importans, comme le cœur, les poumons et le cerveau. Le second étoit de voir comment procédent les phénomènes de l'irritabilité musculaire; parce qu'ayant entendu dire à un célèbre naturaliste, que l'engourdissement avoit pour cause l'altération ou la suspension de cette irritabilité, il m'importoit de savoir si cette assertion étoit vraie. Dans la chambre où se trouvoit la marmotte, le thermomètre étoit à 6 degrés et demi: l'ayant introduit dans le bas ventre, il monta d'un degré, c'est-à-dire à 7 degrés et demi."

"Je trouvai les poumons dans leur état naturel. Le cœur continua à battre pendant plus de trois heures. Les pulsations, d'abord vives et frequentes, s'affoiblirent et se ralentirent peu-à-peu. J'en avois compté de seize à dix-huit par minute au commencement de la première heure; à la fin de la troisième je n'en comptois plus que trois dans le même temps. Les veines du cerveau me parurent gonflées de sang.

"La tête unie au cou ayant été séparée du tronc, je la mis dans un vase avec de l'esprit-de-vin, et j'y remarquai, même après une demi-heure, des mouvemens assez sensibles. Ce fait prouve, ainsi que plusieurs autres dont je parlerai bientôt, que si dans l'état de léthargie conservatrice la vie est beaucoup moins énergique, le principe vital répandu dans les diverses parties, a beaucoup plus de tenacité, et tarde bien plus à s'éteindre."

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* Tome x. p. 453—456.

" Je séparai du corps de l'animal plusieurs morceaux des muscles qui obéissent à la volonté, et je vis avec étonnement que, trois heures après la mort, ils se contractoient fortement chaque fois que je les soumettois à l'action galvanique. Ces mouvemens convulsifs ne se ralentirent qu'au bout de quatre heures.

" Il suit de là que les marmottes tuées pendant qu'elles sont en léthargie, présentent, relativement à l'irritabilité, à peu près les mêmes phénomènes qu'on remarque dans plusieurs animaux à sang froid.

" Pour savoir ensuite si les phénomènes d'irritabilité etoient les mêmes dans l'état de veille et dans celui de léthargie, le 25 de Juin, j'ai fait périr, précisément de la même manière, une seconde marmotte qui étoit éveillée depuis deux mois, et qui faisoit de frequentes courses dans le jardin. Mon thermomètre marquoit ce jour-là 18 degrés : l'ayant introduit dans le ventre de la marmotte au moment où je venois de la decapiter, il s'éleva à 29 degrés.

" Ayant mis le cœur à découvert, comme je l'avois fait dans mon experience du mois de Mars, je comptai d'abord vingt-sept ou vingt-huit pulsations par minute. Ce nombre n'étoit plus que de douze au bout d'un quart d'heure, et de huit, au bout de demi-heure : dans le dix minutes suivantes, il n'y eut plus que quatre pulsations très-foibles par minute, et elles cessèrent totalement dans les dix dernières minutes, c'est-à-dire cinquante minutes après la mort de l'animal ; tandis que le cœur de la marmotte tuée dans l'état de léthargie, donnoit encore quatre légères pulsations par minute, trois heures après que la tête avoit été séparée du corps. Cette grande différence prouve que le principe de l'irritabilité s'accumule pendant la léthargie conservatrice.

" Les chairs musculaires me semblirent plus pâles que celles de la marmotte en léthargie : elles étoient d'abord très sensibles à l'action galvanique ; mais ses signes d'irritabilité s'affoiblirent et disparurent bien plus rapidement. En effet, les chairs musculaires de cette marmotte étoient peu sensibles au bout de deux heures, tandis que dans la marmotte tuée en hiver elles se contractoient fortement au bout de trois heures, et que l'irritabilité ne s'affoiblit notablement que quatre heures après la mort.

" Les chairs des muscles intercostaux et abdominaux conservèrent leur sensibilité au stimulus électrique quelques minutes de plus que celles des membres ; d'où l'on peut conclure que le principe de l'irritabilité se conserve d'avan-

tage dans certaines parties du même animal. Mais ce qui est prouvé jusqu'à l'évidence, c'est que ce principe a bien plus de ténacité dans les chairs de l'animal tué pendant l'état de léthargie, que dans celles de l'animal tué pendant l'état de veille."

This author does not appear to have had any apprehension of the extreme importance of this extraordinary change in the irritability, but merely states it as a fact. Its due value can only be known by observing the dependence of the functions of life on that law of the inverse condition of the respiration and of the irritability, of which so much has already been said. In the hybernating animal the respiration is nearly suspended; had not the irritability become proportionately augmented, the actions of life must have ceased!

3. *Of the Sensibility.*

All the writers upon the subject of hybernation agree in stating that the sensibility is greatly impaired; and it is impossible to commit a greater mistake.

The slightest touch applied to one of the spines of the hedgehog immediately rouses it to draw that deep inspiration of which I have spoken. The merest shake induces a few respirations in the bat. The least disturbance, in fact, is felt, as is obvious from its effect in inducing motion in the animal.

It is from the misconception on this point that the error has arisen, that the respiration is not absolutely suspended in hybernation. This function has been so readily excited, through the medium of an unimpaired sensibility, that the event has been considered as appertaining to the state of hybernation.

In fact, the sensibility is in nearly the same condition in hybernation as in ordinary sleep.

It must appear extraordinary that with an unimpaired sensibility there can co-exist a suspended respiration. Why is not this suspension of respiration painful in the hybernating, as in other animals? And why is not the animal roused, by this pain, from its slumbers, if its sensibility be only slightly impaired?

But we should first ask, what are the precise seat and source of that pain which is felt during the suspension of respiration? These are, I think, demonstrably, the heart, and an impeded circulation through this organ. If, therefore, the circulation through the heart be not obstructed, there will be no painful sensation. Now it is precisely the peculiar property of hybernation, that the circulation through the heart is *not* interrupted, although the respiration be suspended. This topic is reserved, however, for a subsequent part of this paper. It is simply stated in this place as a fact, to show that the painful feelings supposed to arise from suspended respiration in hybernation, do not exist; and that the difficulty of supposing a suspended state of the respiration with an unimpaired sensibility, is, in this manner, entirely removed.

The sensorial functions, on the other hand, are nearly suspended. This is proved by the suspension of respiration, which is immediately renewed, for a time, on exciting the animal. It is further proved by the fact, that although the animal coils itself up when touched, it immediately relaxes into the former position; whereas when it is awake, the impression of an external object induces a state of contraction and immobility which is continued for some time,—probably as long as the sense of fear continues. When the hedgehog, coiled up in its state of activity, is thrown into water, it immediately relaxes itself, from fear, and betakes itself to swimming; in the state of lethargy, on the other hand, no fear appears to be excited under such circumstances, and the animal would probably remain still and quiet for a very considerable period, if its sensibility were not acted upon by the contact of the water.

4. *Of the Muscular Motility.*

The motility of the muscles, in true hybernation, is, like the sensibility, unimpaired. Those physiologists who have asserted the contrary, have, as will be shown shortly, mistaken the phenomena of torpor from cold, for those of true hybernation.

If the hedgehog in a state of the most perfect lethargy, uncomplicated with torpor, be touched, its respiration is resumed, and it coils itself up more forcibly than before. The dormouse, in similar circumstances, unfolds itself; and the bat moves variously. Not the slightest stiffness is observed. The hedgehog, when roused, walks about, and does not stagger as has been asserted. The bat speedily takes to the wing, and flies about with great activity, although exhaustion and death may subsequently result from the experiment. The phenomena are similar to those of awaking from natural sleep. Insensibility, impaired motility, stiffness, lameness, &c. belong to torpor, and not to true hybernation.

5. *Of the Circulation.*

The wing of the bat affords an admirable opportunity of observing the condition of the circulation during hybernation. But it requires peculiar management. If the animal be taken from its cage, and the wing extended under the microscope, it is roused by the operation, and its respiratory and other movements are so excited, that all accurate observation of the condition of the circulation in the minute vessels is completely frustrated. Still greater caution is required in this case, than even in the observation of the respiration and temperature.

After some fruitless trials, I at length succeeded perfectly in obtaining a view of the minute circulation undisturbed. Having placed the animal in its state of hybernation, in a little box of mahogany, I gently drew out its wing through a crevice made in the side of the box; I fixed the tip of the extended wing between portions of cork; I then attached the box and the cork to a piece of plate-glass; and, lastly, I left the animal in this situation, in a cold atmosphere, to resume its lethargy.

I could now quietly convey the animal ready prepared, and place it in the field of the microscope without disturbing its slumbers, and observe the condition of the circulation.

In this manner I have ascertained, that, although the respiration be suspended, the circulation continues uninterruptedly. It is slow in the minute arteries and veins; the beat of the heart is regular, and generally about twenty-eight times in the minute.

We might be disposed to view the condition of the circulation in the state of hybernation as being reptile, or analogous to that of the batrachian tribes. But when we reflect that the respiration is nearly, if not totally, suspended, and that the blood is venous*, we must view the condition of the circulation as in a lower condition still, and, as it were, sub-reptile. It may, indeed, be rather compared to that state of the circulation which is observed in the frog from which the brain and spinal marrow have been removed by minute portions at distant intervals*.

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* M. PRUNELLE observes, "En comparant le sang de deux chauve-souris auxquelles j'avois ouvert les carotides, à l'une pendant son engourdissement et à l'autre dans l'état de veille, j'ai trouvé celui de la dernière beaucoup plus vermeil." Annales du Museum, tome xviii. p. 28.

In fact, in the midst of a suspended respiration, and an impaired condition of some other functions, one vital property is augmented. This is the irritability, and especially the irritability of the left side of the heart. The left side of the heart, which is, in the hybernating animal, in its state of activity, as in all the other mammalia, only arterio-contractile, becomes veno-contractile.

This phenomenon is one of the most remarkable presented to me in the whole animal kingdom. It forms the single exception to the most general rule, amongst animals which possess a double heart. It accounts for the possibility of immersion in water or a noxious gas, without drowning or asphyxia; and it accounts for the possibility of a suspended respiration, without the feeling of oppression or pain, although sensation be unimpaired. It is, in a word, this peculiar phenomenon, which, conjoined with the peculiar effect of sleep in inducing diminished respiration in hybernating animals, constitutes the susceptibility and capability of taking on the hybernating state. On the other hand, as the rapid circulation of a highly arterialized blood in the brain and spinal marrow of birds probably conduces to their activity, the slow circulation of a venous blood, doubtless contributes to the lethargy of the hybernating animal.

6. *Of the Digestion.*

There is much difference in the powers of digestion, and in the fact of omitting to take food, in the hybernation of different animals. The bat, being insectivorous, would awake in vain; no food could be found: the hedgehog might obtain snails or worms, if the ground were not very hard from frost: the dormouse would find less difficulty in meeting with grain and fruits. We accordingly observe a remarkable difference in the habits of awaking from their lethargy or hybernation, in these different animals.

I have observed no disposition to awake at all in the bat, except from external warmth or excitement. If the temperature be about 40° or 45°, the hedgehog, on the other hand, awakes, after various intervals of two, three, or four days passed in lethargy, to take food; and again returns to its state of hybernation. The dormouse, under similar circumstances, awakes daily.

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* Essay on the Circulation, pp. 136—141.

Proportionate to the disposition to awake and take food, is the state of the functions of the stomach, bowels and kidneys. The dormouse and the hedgehog pass the fæces and urine in abundance during their intervals of activity. The bat is scarcely observed to have any excretions during its continued lethargy.

In the dormouse and the hedgehog, the sense of hunger appears to rouse the animal from its hybernation, whilst the food taken conduces to a return of the state of lethargy. It has already been observed, that there are alternations between activity and lethargy in this animal, with the taking of food, in temperatures about 40° or 45°. Nevertheless, abstinence doubtless conduces to hybernation, by rendering the system more susceptible of the influence of cold, in inducing sleep and the loss of temperature. The hedgehog, which awakes from its hybernation, and does not eat, returns to its lethargy sooner than the one which is allowed food.

III. *Of Torpor from Cold.*

It is highly important, and essential to the present investigation, to distinguish that kind of torpor which may be produced by cold in any animal, from true hybernation, which is a property peculiar to a few species. The former is attended by a benumbed state of the sentient nerves, and a stiffened condition of the muscles; it is the direct and immediate effect of cold, and even in the hybernating animal is of an injurious and fatal tendency; in the latter, the sensibility and motility are unimpaired, the phenomena are produced through the medium of sleep; and the effect and object are the preservation of life.

Striking as these differences are, it is certain that the distinction has not always been made by former observers. In all the experiments which have been made, with artificial temperatures especially, it is obvious that this distinction has been neglected.

True hybernation is induced by temperatures only moderately low. All hybernating animals avoid exposure to extreme cold. They seek some secure retreat, make themselves nests or burrows, or congregate in clusters, and, if the season prove unusually severe, or if their retreat be not well chosen and they be exposed in consequence to excessive cold, many become benumbed, stiffen, and die.

In our experiments upon hybernation we should imitate nature's operations. Would any one imagine that the following detail contained the account of an experiment upon this subject? "Le 31 Janvier," says M. SAISSY, "à trois heures du soir, la température atmosphérique étant à 1°·25 au-dessous de zéro, celle d'un hérisson engourdi profondément à 3°·50 au dessus, j'enfermai ce quadrupède dans un bocal de verre entouré de toute part d'une mixtion de glace et de muriate de soude. L'excés du froid le réveilla d'abord, mais trois heures ont suffi pour le replonger dans une profonde torpeur.

"J'avais placé l'animal de manière que je pouvais répéter, autant que je le jugeais nécessaire, les expériences thermométriques. Dès que sa température eut baissé jusqu'à zéro, (ce ne fut qu'à 2 heures du matin) je le retirai du bocal et le placai dans une température de 12° et plus au dessus de la glace; mais l'animal était mort*."

To induce true hybernation, it is quite necessary to avoid extreme cold; otherwise we produce the benumbed and stiffened condition to which the term torpor or torpidity may be appropriated. I have even observed that methods which secure moderation in temperature, lead to hybernation: hedgehogs supplied with hay or straw; and dormice, supplied with cotton wool, make themselves nests and become lethargic; when others, to which these materials are denied, and which are consequently more exposed to the cold, remain in a state of activity. In these cases, warmth or moderated cold actually concur to produce hybernation†.

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* Recherches sur les Animaux hybernans, par M. J. A. SAISSY: pp. 13, 14.

† M. CUVIER observes of the Tenrec, "Ce sont des animaux nocturnes qui passent trois mois de l'année en léthargie, quoique habitants de la zône torride. BRUGUIÈRE assure même que c'est pendant les plus grandes chaleurs qu'ils dorment." Règne Animal, Ed. 1829, tome i. p. 125. This account, however, does not agree with that of Mr. TELFAIR given in the Proceedings of the Zoological Society, No. viii. p. 89. Mr. TELFAIR states, "In the Mauritius they sleep through the greater part of the winter, from April to November, and are only to be found when the summer heat is felt, which being generally ushered in by an electric state of the atmosphere, the negroes (with whom they are a favourite food,) say they are awakened by the peals of thunder which precede the summer storms, or 'pluies d'orages.' Even in summer they are not often seen beyond the holes in which they burrow, except at night. Their favourite haunts are among the old roots of clumps of bamboos."

When we read of insensibility, of a stiffened state of the muscles, and of a cessation of the circulation, as obtaining in hybernation, we may be certain that a state of torpor has been mistaken for that condition. The actually hybernating animal exposed to continued severe cold, is, as M. Saissy correctly observes, first roused from this state of ease and preservation, into a painful activity, and then plunged into a fatal torpor.

This subject will come to be considered in a subsequent part of this inquiry, in which I purpose to trace the effects of cold in changing the relative quantity of respiration and degree of the irritability in animals of different ages which do not hybernate; in the meantime, the accurate distinction between mere torpor, which may occur in any animal, and which is a destructive state, from true hybernation, which is preservative, and the peculiarity of certain animals, will enable us to correct many inaccuracies into which Legallois*, M. Edwards†, and other physiologists have fallen.

IV. *Of Reviviscence.*

Not the least interesting of the phenomena connected with hybernation, are those of reviviscence. Hybernation induces a state of irritability of the left side of the heart, which, with high respiration and an arterialized blood, would be incompatible with life. Respiration suddenly restored, and permanently excited, is, therefore, as destructive as its privation in other circumstances.

All those bats which were sent to me from distant parts of the country died. The continued excitement from the motion of the coach, keeping them in a state of respiration, the animal perished. One bat had, on its arrival, been roused so as to fly about. Being left quiet, it relapsed into a state of hybernation. The excitement being again repeated the next day, it again flew about the room; on the succeeding day it was found dead.

It is in accordance with this law, that we observe hybernating animals adopting various measures to secure themselves from frequent sources of disturbance and excitement. They choose sheltered situations, as caverns, burrows, &c., secure from the rapid changes and the inclemencies of the weather and season. Many form themselves nests; others congregate together. The hedgehog and the dormouse roll themselves up into a ball. The common bat suspends itself by the claws of its hinder feet, with its head dependent, generally in clusters; the horseshoe bat, (ferrum equinum,) spreads its wings so as to embrace and protect its fellows.

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* Œuvres de Legallois: Paris, 1824, p. 282.

† Agens Physiques, pp. 292, 148.

All these circumstances are obviously designed to prevent disturbed hybernation.

In the depth of caverns, and other situations sheltered from changes of temperature in the atmosphere, the calls of hunger are probably the principal cause of reviviscence in the spring. The other causes of reviviscence are the return of warmth and external excitements: it is interesting to observe and trace the gradual return of respiration in the former case, and of the temperature of the animal in the latter.

If the hybernating hedgehog be touched even very gently, it draws a deep breath, and then continues to breathe for a short time. If this excitement be repeated, the animal is permanently roused, and its temperature raised. If the temperature of the atmosphere be augmented, the respiration is gradually excited, and the animal is gradually restored to its state of activity.

If a hybernating animal be excited in a very cold atmosphere, its temperature rises variously, and then falls. A bat was perfectly lethargic in a temperature of 36°. A fine thermometer, with a cylindrical bulb, was introduced into its stomach; it rose to 39°. One hour afterwards, the animal not being further disturbed, the respiration was rapid, and the temperature in the stomach 95°. Shortly afterwards the temperature was 90°. The minute circulation was pretty good, and pulsatory in the arteries, the heart beating from twenty-eight to thirty-six times in the minute.

In another bat, in an atmosphere of the temperature of 36°, the thermometer in the stomach rose to 39°. The animal being continually excited, the temperature rose to 65°, but speedily fell to 60°.

The animal excited and revived in this manner, is in a state of exhaustion and inanition. It is incapable of maintaining its temperature if exposed to cold, and will die unless it repass into the state of hybernation. It may be compared to the case of the mouse deprived of food in the following experiment of Mr. HUNTER. "A mouse was put into a cold atmosphere of 13° for an hour, and then the thermometer was introduced as before; but the animal had lost heat, for the quicksilver at the diaphragm was carried only to 83°, in the pelvis to 78°."

"In order to determine whether an animal that is awakened has the same powers, with respect to preserving heat and cold, as one that is vigorous and strong, I weakened a mouse by fasting, and then introduced the bulb of the thermometer into its belly; the bulb being at the diaphragm, the quicksilver rose to 97°; in the pelvis to 95°, being two degrees colder than the strong mouse: the mouse being put into an atmosphere as cold as the other, and the thermometer again introduced, the quicksilver stood at 79° at the diaphragm, and at 74° in the pelvis.

"In this experiment, the heat at the diaphragm was diminished 18°, in the pelvis 21°.

"This greater diminution of heat in the second than in the first, we may suppose proportional to the decreased power of the animal, arising from want of food*."

But extreme cold alone, by a painful effect induced on the sentient nerves, rouses the hybernating animal from its lethargy, as has been remarked already, and is illustrated by the following experiments of HUNTER. "Having brought a healthy dormouse, which had been asleep from the coldness of the atmosphere, into a room in which there was a fire, (the atmosphere at 64°,) I introduced the thermometer into its belly, nearly at the middle, between the thorax and pubis, and the quicksilver rose to 74° or 75°; turning the bulb towards the diaphragm, it rose to 80°; and when I applied it to the liver, it rose to 81½°.

"The mouse being placed in an atmosphere at 20°, and left there half an hour, when taken out was very lively, even much more so than when put in. Introducing the thermometer into the lower part of the belly, the quicksilver rose to 91°; and upon turning it up to the liver, to 93°.

"The animal being replaced in the cold atmosphere at 30°, for an hour, the thermometer was again introduced into the belly; at the liver it rose to 93°; in the pelvis to 92°; the mouse continuing very lively.

"It was again put back into an atmosphere cooled to 19°, and left there an hour; the thermometer at the diaphragm was 87°; in the pelvis 83°; but the animal was now less lively.

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* Animal Œconomy, pp. 114, 115.

"Having been put into its cage, the thermometer being placed at the diaphragm, in two hours afterwards, was at 93° *."

In these experiments the animals appear to have been roused partly by the state of the wound in the abdomen, but chiefly by the extreme cold. They can scarcely, however, be considered as experiments upon hybernation, however interesting they may be in reference to reviviscence from that state.

The fact of the fatal influence of excited respiration during the augmented irritability of hybernation, contrasted with the similar fatal effect of suspended respiration, during the diminished irritability of the state of activity, will illustrate many of the causes, kinds, and phenomena of death. Do not these resolve themselves, in fact, into irritability insufficiently or excessively excited?

Recapitulation.

The object of this paper has been to treat of the singular phenomena of hybernation, and especially to point out the remarkable application of the law stated in my former paper, to the active and lethargic states of the hybernating animal.

1. The natural sleep of the hybernating animal differs greatly, yet only in degree, from the sleep of any other animal.

2. This sleep passes insensibly into the state of true hybernation, which is more profound, as the blood loses its arterial character; for

3. In hybernation, the respiration and the evolution of heat are nearly suspended.

4. The irritability is, at the same time, singularly augmented; and the animal bears proportionately the privation of air.

5. The nervous sensibility and the muscular motility are unimpaired.

6. There is the singular phenomenon of this unimpaired sensibility, and the capability of bearing the privation of air without pain; a fact which receives an interesting and perfect explanation from the additional fact of the augmented irritability or veno-contractility of the left side of the heart.

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* Animal Œconomy, pp. 111, 112.

7. There is an important distinction between true hybernation and torpor from cold, not attended to by physiologists.

8. Severe cold, like all other causes of pain, rouses the hybernating animal from its lethargy ; and, if continued, induces the state of torpor.

In conclusion, one of the most general effects of sleep, is to impair the respiration, and with that function, the evolution of animal temperature. The impaired state of the respiration, induces a less arterial condition of the blood, which then becomes unfit for stimulating the heart; accumulation of the blood takes place in the pulmonary veins and left auricle; a sense of oppression is induced, and the animal is either roused to draw a deep sigh, or awakes altogether.

Such are the phenomena in animals in which the heart has not the faculty of taking on an augmented state of irritability, with this lessened degree of stimulus. But in those animals which do possess this faculty, a property which constitutes the power of hybernation, the heart continues the circulation of the blood, more slowly indeed, but not less perfectly, although its arterial character be diminished and its stimulant property impaired. No repletion of the pulmonary veins and of the left auricle, no sense of oppression is induced, and the animal is not roused; the respiration continues low, the temperature falls, and the animal can bear, for a short period, the abstraction of atmospheric air.

All the phenomena of hybernation originate, then, in the susceptibility of augmented irritability. The state of sleep, which may be viewed as the first stage of hybernation, induces an impaired degree of respiration. This would soon be attended with pain, if the irritability of the heart were not at the same time augmented, so as to carry on the circulation of a less arterial blood, and the animal would draw a deep sigh—would augment its respiration, or awake. Occasional sighs are, indeed, observed in the sleep of all animals, except the hybernating. In these, the circulation goes on uninterruptedly, with a diminished respiration, by the means of an augmented irritability. There is no stagnation of the blood at the heart; consequently, no uneasiness; and the animal becomes more and more lethargic, as the circulation of a venous blood is more complete. This lethargy is eventually interrupted by circumstances which break ordinary sleep, as external stimuli, or the calls of appetite.

Moderate cold disposes to sleep,—to lethargy. But severer cold induces a different condition of the system,—that of torpor. Sleep is the *medium* between such moderate cold and the phenomena of hybernation; torpor is the *immediate* effect of the severer degrees of cold.

This investigation naturally leads to that of the comparative conditions of the respiration and of the irritability, in the pupa and perfect states of some species of the insect tribes. There is much reason to suppose that these states are respectively similar to those of lethargy and activity in the hybernating animal.

XVII. *Researches in Physical Astronomy.* By J. W. Lubbock, *Esq. V.P. and Treas. R.S.*

Read June 7, 1832.

I SUBJOIN some further developments in the Theory of the Moon, which I have thought it advisable to give at length, in order to save the trouble of the calculator and to avoid the danger of mistake, although they may be obtained with great readiness and facility by means of the Table which I have given for the purpose.

While on the one hand it seems desirable to introduce into the science of Physical Astronomy a greater degree of uniformity, by bringing to perfection a Theory of the Moon, founded on the integration of the equations which are used in the planetary theory, it seems also no less important to complete in the latter the method hitherto applied solely to the periodic inequalities. Hitherto those terms in the disturbing function which give rise to the secular inequalities have been detached, and the stability of the system has been inferred by means of the integration of certain equations, which are linear when the higher powers of the eccentricities are neglected, and from considerations founded on the variation of the elliptic constants.

The stability of the system may, I think, also be inferred from the expressions which result at once from the direct integration of the differential equations. In fact, in order that the system may be stable, it is necessary that none of the angles under the sign *sine* or *cosine* be imaginary, which terms would then be converted into exponentials, and be subject to indefinite increase. In the lunar theory, the arbitrary quantities being determined with that view, according to the method here given, the angles which are introduced may be reduced to the difference of the mean motions of the sun and moon, their mean anomalies and the argument of the moon's latitude *.

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* So that however far the approximation be carried, all the arguments, in the expressions of r, s, and λ are of the form, $it \pm kx \pm lz \pm my$; i, k, l, and m being some whole numbers.

This being the case, no imaginary angles are introduced, if the quantities c and g are rational. This theory, which does not seem to be limited by the direction of the moon's motion, and which may be extended without difficulty, already embraces the terms which are included in the secular inequalities, and which are derived from the constant part of *R* carried to the order of the squares of the eccentricities. Generally when the method of the variation of constants is employed to determine any inequalities, the development of *R* must be carried one degree further, as regards the eccentricities, than the degree which is required of the inequalities sought.

The equation for determining the coefficients of the expression for the reciprocal of the radius vector is,

$$\frac{\mathrm{d}^2 . \mathrm{r}^2}{2\,\mathrm{d}\,t^2} - \frac{\mathrm{d}^2 . \mathrm{r}^3\,\delta\,\frac{1}{r}}{\mathrm{d}\,t^2} + \frac{3\,\mathrm{d}^2 . r^4 \left(\delta\,\frac{1}{r}\right)^2}{2\,\mathrm{d}\,t^2} - \frac{2\,\mathrm{d}^2 . \mathrm{r}^5 \left(\delta\,\frac{1}{r}\right)^3}{\mathrm{d}\,t^2} - \frac{\mu}{r} + \frac{\mu}{a} + 2\int \mathrm{d}\,R + r\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,r}\right) = 0$$

$$\begin{aligned} \mathrm{r}^3\,\delta . \frac{1}{r} - \frac{3}{2}\left(r\,\delta\,\frac{1}{r}\right)^2 = \Bigg\{ & \left\{1 + 3\,e^2\left(1 + \frac{e^2}{8}\right)\right\} r_1 - \frac{3\,e^2}{2}\left(1 + \frac{3}{8}\,e^2\right)(r_3 + r_4) \\ & - \frac{3}{2}\left\{2\,r_0\,r_1 + e^2\,(r_3 + r_4)\,r_2 + e_{\prime}^2\,(r_6 + r_7)\,r_5\right\} \Bigg\} \cos 2\,t + \&c. \\ & - 3\,e^2\left\{2\,r_1\,r_2 + 2\,r_0\,r_3 + 2\,r_0\,r_4\right\} \end{aligned}$$

$r_n{}'$ being the coefficient corresponding to the n^{th} argument in the development of $\mathrm{r}\,\delta\,\frac{1}{r}$. The development of $\mathrm{r}^3\,\delta\,\frac{1}{r}$ is easily deduced from that of $\mathrm{r}\,\delta\,\frac{1}{r}$ given in the Phil. Trans. 1832, Part I. p. 3, and that of $\left(\mathrm{r}\,\delta\,\frac{1}{r}\right)^2$ from that of $\left(\delta\,\frac{1}{r}\right)^2$, p. 4. If $\mathfrak{r}_n$ is that part of the coefficient of the n^{th} argument in the development of the quantity $\mathrm{r}^3\,\delta\,\frac{1}{r} - \frac{3}{2}\left(\mathrm{r}\,\delta\,\frac{1}{r}\right)^2$ which is independent of r_n, with a contrary sign;

$$\begin{aligned} \mathfrak{r}_1 = \frac{3\,e^2}{2}\left(1 + \frac{3}{8}\,e^2\right)(r_3 + r_4) + \frac{3}{2}\left\{2\,r_0\,r_1 + e^2\,(r_3 + r_4)\,r_2 + e_{\prime}^2\,(r_6 + r_7)\,r_5\right\} \\ - 3\,e^2\left\{2\,r_1\,r_2 + 2\,r_0\,r_3 + 2\,r_0\,r_4\right\} \end{aligned}$$

$$\begin{aligned} \mathfrak{r}_2 = \frac{3}{2}\left(1 + \frac{3}{8}\,e^2\right)(2\,r_0 + e^2\,r_8) + \frac{3}{2}\left\{(r_4 + r_3)\,r_1 + 2\,r_0\,r_2\right\} \\ - 6\left\{r_0^2 + \frac{r_1^2}{2} + \frac{e^2\,r_3^2}{2} + \&c.\right\} \end{aligned}$$

$$\mathfrak{r}_3 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_9 + r_1) + \frac{3}{2}\left\{r_1 r_2 + 2 r_0 r_3\right\}$$

$$- 3\,\{2 r_0 r_1 + e^2 (r_3 + r_4)\, r_2 + e_{,}^2 (r_6 + r_7)\, r_5\}$$

$$\mathfrak{r}_4 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(r_1 + e^2 r_{10}) + \frac{3}{2}\left\{r_1 r_2 + 2 r_0 r_4\right\}$$

$$- 3\,\{2 r_0 r_1 + e^2 (r_3 + r_4)\, r_2 + e_{,}^2 (r_6 + r_7)\, r_5\}$$

$$\mathfrak{r}_5 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{14} + e^2 r_{11}) + \frac{3}{2}\left\{r_1 r_7 + r_1 r_6 + 2 r_0 r_5\right\}$$

$$\mathfrak{r}_6 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{12} + e^2 r_{16}) + \frac{3}{2}\left\{r_5 r_1 + 2 r_0 r_6\right\}$$

$$\mathfrak{r}_7 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{15} + e^2 r_{13}) + \frac{3}{2}\left\{r_5 r_1 + 2 r_0 r_7\right\}$$

$$\mathfrak{r}_8 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(2 r_0 + r_2 + e^2 r_{20}) + \frac{e^2 r_2}{16} + \frac{3}{2}\left\{r_2^2 + r_4 r_3 + r_1 r_9 + r_1 r_{10}\right\}$$

$$- 3\left\{2 r_0^2 + r_1^2 + (r_4 + r_3)\, r_1 + 2 r_0 r_2\right\} + 3\left\{r_0^2 + \frac{r_1^2}{2}\right\}$$

$$\mathfrak{r}_9 = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{21} + r_3) + \frac{e^2}{16} r_4 + \frac{3}{2}\left\{r_2 r_3 + 2 r_0 r_9\right\}$$

$$- 3\left\{r_1 r_2 + 2 r_0 r_3\right\} + \frac{3}{2}\left\{2 r_0 r_1 + e^2 r_3 r_2\right\}$$

$$\mathfrak{r}_{10} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(r_4 + e^2 r_{22}) + \frac{e^2}{16} r_3 + \frac{3}{2}\left\{r_4 r_2 + 2 r_0 r_{10}\right\}$$

$$- 3\left\{r_1 r_2 + 2 r_0 r_4\right\} + \frac{3}{2}\left\{2 r_0 r_1 + e^2 r_3 r_2\right\}$$

$$\mathfrak{r}_{11} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(r_5 + e^2 r_{23}) + \frac{3}{2}\left\{r_1 r_{13} + r_1 r_{12} + r_2 r_5 + r_6 r_4 + r_3 r_7 + 2 r_0 r_{11}\right\}$$

$$- 3\,\{r_1 r_7 + r_1 r_6 + 2 r_0 r_5\}$$

$$\mathfrak{r}_{12} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{24} + r_6) + \frac{3}{2}\left\{r_{11} r_1 + r_2 r_6 + r_5 r_3 + 2 r_0 r_{12}\right\} - 3\left\{r_5 r_1 + 2 r_0 r_6\right\}$$

$$\mathfrak{r}_{13} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(r_7 + e^2 r_{25}) + \frac{3}{2}\left\{r_{11} r_1 + r_2 r_7 + r_5 r_4 + 2 r_0 r_{13}\right\} - 3\left\{r_5 r_1 + 2 r_0 r_7\right\}$$

$$\mathfrak{r}_{14} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{26} + r_5) + \frac{3}{2}\left\{r_{16} r_1 + r_{15} r_1 + r_2 r_5 + r_6 r_3 + r_7 r_4 + 2 r_0 r_{14}\right\}$$

$$- 3\,\{r_1 r_7 + r_1 r_6 + 2 r_0 r_5\}$$

$$\mathfrak{r}_{15} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(e^2 r_{27} + r_7) + \frac{3}{2}\left\{r_{14} r_1 + r_2 r_7 + r_5 r_3\right\} - 3\left\{r_5 r_1 + 2 r_0 r_7\right\}$$

$$\mathfrak{r}_{16} = \frac{3}{2}\left(1 + \frac{3}{8}e^2\right)(r_6 + e^2 r_{28}) + \frac{3}{2}\left\{r_{14} r_1 + r_2 r_6 + r_5 r_4\right\} - 3\left\{r_5 r_1 + 2 r_0 r_6\right\}$$

$$\mathfrak{r}_{17}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)(e^2r_{32}+e^2r_{29})+\frac{3}{2}\left\{r_5^2+r_7r_6+r_1r_{18}+r_1r_{19}\right\}$$

$$\mathfrak{r}_{18}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)(e^2r_{30}+e^2r_{34})+\frac{3}{2}\left\{r_{17}r_1+r_5r_6\right\}$$

$$\mathfrak{r}_{19}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)(e^2r_{33}+e^2r_{31})+\frac{3}{2}\left\{r_{17}r_1+r_7r_5\right\}$$

$$\mathfrak{r}_{20}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_8+\frac{1}{8}r_0 \qquad \mathfrak{r}_{21}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_9+\frac{1}{16}r_1$$

$$\mathfrak{r}_{22}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{10}+\frac{1}{16}r_1 \qquad \mathfrak{r}_{23}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{11}$$

$$\mathfrak{r}_{24}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{12} \qquad \mathfrak{r}_{25}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{13}$$

$$\mathfrak{r}_{26}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{14} \qquad \mathfrak{r}_{27}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{15} \qquad \mathfrak{r}_{28}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{16}$$

$$\mathfrak{r}_{29}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{17} \qquad \mathfrak{r}_{30}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{18} \qquad \mathfrak{r}_{31}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{19}$$

$$\mathfrak{r}_{32}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{17} \qquad \mathfrak{r}_{33}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{19} \qquad \mathfrak{r}_{34}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{18}$$

$$\mathfrak{r}_{35}=0 \qquad \mathfrak{r}_{36}=0 \qquad \mathfrak{r}_{37}=0$$

$$\mathfrak{r}_{38}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{20}+\frac{1}{16}r_2 \qquad \mathfrak{r}_{39}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{21}+\frac{1}{16}r_3$$

$$\mathfrak{r}_{40}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{22}+\frac{1}{16}r_4 \qquad \mathfrak{r}_{41}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{23}+\frac{1}{16}r_5$$

$$\mathfrak{r}_{42}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{24}+\frac{1}{16}r_6 \qquad \mathfrak{r}_{43}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{25}+\frac{1}{16}r_7$$

$$\mathfrak{r}_{44}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{26}+\frac{1}{16}r_5 \qquad \mathfrak{r}_{45}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{27}+\frac{1}{16}r_7$$

$$\mathfrak{r}_{46}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{28}+\frac{1}{16}r_6 \qquad \mathfrak{r}_{47}=\frac{3}{2}\left(1+\frac{3}{8}e^2\right)r_{29}$$

Let R_n be the coefficient corresponding to the n^{th} argument in the development of $a R + a \delta R$, $m R'_n$ the coefficient corresponding to the n^{th} argument in the development of $a \delta \mathrm{d} R$ with its sign changed, Phil. Trans. 1832, p. 161, so that, for example, when the square of the disturbing force is neglected,

$R_1 = -\frac{3}{4}\frac{m_{\prime}}{\mu}\frac{a^3}{a_{\prime}^3}$ then

$$r_1\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-2m)^2}{(2-2m)^2-1}\mathfrak{r}_1-\frac{2}{(2-2m)^2-1}\left\{\left\{\frac{2}{2-2m}+1\right\}R_1+\frac{m}{2-2m}R_1'\right\}$$

$$c^2\left\{1-\frac{e^2}{8}-\mathfrak{r}_2\right\}=1-\frac{e^2}{8}-2\left\{\left\{\frac{1}{c}+1\right\}R_2+\frac{m}{c}R_2'\right\}$$

$$r_3\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-2m-c)^2}{(2-2m-c)^2-1}\mathrm{r}_3$$
$$-\frac{2}{(2-2m-c)^2-1}\left\{\left\{\frac{2-c}{2-2m-c}+1\right\}R_3+\frac{m}{2-2m-c}R_3'\right\}$$

$$r_4\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-2m+c)^2}{(2-2m+c)^2-1}\mathrm{r}_4$$
$$-\frac{2}{(2-2m+c)^2-1}\left\{\left\{\frac{2+c}{2-2m+c}+1\right\}R_4+\frac{m}{2-2m+c}R_4'\right\}$$

$$r_5\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{m^2}{m^2-1}\mathrm{r}_5-\frac{2}{m^2-1}\left\{R_5+R_5'\right\}$$

$$r_6\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-3m)^2}{(2-3m)^2-1}\mathrm{r}_6-\frac{2}{(2-3m)^2-1}\left\{\left\{\frac{2}{2-3m}+1\right\}R_6+\frac{m}{2-3m}R_6'\right\}$$

$$r_7\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-m)^2}{(2-m)^2-1}\mathrm{r}_7-\frac{2}{(2-m)^2-1}\left\{\left\{\frac{2}{2-m}+1\right\}R_7+\frac{m}{2-m}R_7'\right\}$$

$$c^2\left\{1-\frac{e^2}{3}-2r_8-2\mathrm{r}_8\right\}=1-\frac{e^2}{3}-2\left\{\left\{\frac{1}{c}+1\right\}R_8+\frac{m}{2c}R_8'\right\}$$

$$r_9\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-2m-2c)^2}{(2-2m-2c)^2-1}\mathrm{r}_9$$
$$-\frac{2}{(2-2m-2c)^2-1}\left\{\left\{\frac{2-2c}{2-2m-2c}+1\right\}R_9+\frac{m}{2-2m-2c}R_9'\right\}$$

$$r_{10}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-2m+2c)^2}{(2-2m+2c)^2-1}\mathrm{r}_{10}$$
$$-\frac{2}{(2-2m+2c)^2-1}\left\{\left\{\frac{2+2c}{2-2m+2c}+1\right\}R_{10}+\frac{m}{2-2m+2c}R_{10}'\right\}$$

$$r_{11}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(c+m)^2}{(c+m)^2-1}\mathrm{r}_{11}-\frac{2}{(c+m)^2-1}\left\{\left\{\frac{c}{c+m}+1\right\}R_{11}+\frac{m}{c+m}R_{11}'\right\}$$

$$r_{12}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-c-3m)^2}{2-c-3m)^2-1}\mathrm{r}_{12}$$
$$-\frac{2}{(2-c-3m)^2-1}\left\{\left\{\frac{2-c}{2-c-3m}+1\right\}R_{12}+\frac{m}{2-c-3m}R_{12}'\right\}$$

$$r_{13}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-m+c)^2}{(2-m+c)^2-1}\mathrm{r}_{13}$$
$$-\frac{2}{(2-m+c)^2-1}\left\{\left\{\frac{2+c}{2-m+c}+1\right\}R_{13}+\frac{m}{2-m+c}R_{13}'\right\}$$

$$r_{14}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(c-m)^2}{(c-m)^2-1}\mathrm{r}_{14}-\frac{2}{(c-m)^2-1}\left\{\left\{\frac{c}{c-m}+1\right\}R_{14}+\frac{m}{c-m}R_{14}'\right\}$$

$$r_{15}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-m-c)^2}{(2-m-c)^2-1}\mathfrak{r}_{15}$$

$$-\frac{2}{(2-m-c^2)-1}\left[\left\{\frac{2-c}{2-m-c}+1\right\}R_{15}+\frac{m}{2-m-c}R_{15}'\right]$$

$$r_{16}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-3m+c)^2}{(2-3m+c)^2-1}\mathfrak{r}_{16}$$

$$-\frac{2}{(2-3m+c)^2-1}\left[\left\{\frac{2+c}{2-3m+c}+1\right\}R_{16}+\frac{m}{2-3m+c}R_{16}'\right]$$

$$r_{17}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{4m^2}{4m^2-1}\mathfrak{r}_{17}-\frac{2}{4m^2-1}\left\{R_{17}+R'_{17}\right\}$$

$$r_{18}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(2-4m)^2}{(2-4m)^2-1}\mathfrak{r}_{18}$$

$$-\frac{2}{(2-4m)^2-1}\left[\left\{\frac{2}{2-4m}+1\right\}R_{18}+\frac{m'}{2-4m}R'_{18}\right]$$

$$r_{19}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{4}{3}\mathfrak{r}_{19}-\frac{2}{3}\left\{2R_{19}+\frac{m}{2}R'_{19}\right\}$$

$$r_{101}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(1-m)^2}{(1-m)^2-1}\mathfrak{r}_{101}$$

$$-\frac{1}{(1-m)^2-1}\left[\left\{\frac{2}{1-m}+3\right\}R_{101}+\frac{2m}{1-m}R_{101}'\right]$$

$$r_{102}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(1-m-c)^2}{(1-m-c)^2-1}\mathfrak{r}_{102}$$

$$-\frac{1}{(1-m-c)^2-1}\left[\left\{\frac{2(1-c)}{1-m-c}+3\right\}R_{102}+\frac{2m}{1-m-c}R_{102}'\right]$$

$$r_{103}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(1-m+c)^2}{(1-m+c)^2-1}\mathfrak{r}_{103}$$

$$-\frac{1}{(1-m+c)^2-1}\left[\left\{\frac{2(1+c)}{(1-m+c)}+3\right\}R_{103}+\frac{2m}{1-m+c}R_{103}'\right]$$

$$r_{104}\left\{1+3e^2\left(1+\frac{e^2}{8}\right)\right\}=\frac{(1-2m)^2}{(1-2m)^2-1}\mathfrak{r}_{104}$$

$$-\frac{1}{(1-2m)^2-1}\left[\left\{\frac{2}{1-2m}+3\right\}R_{104}+\frac{2m}{1-2m}R_{104}'\right]$$

$$m={\cdot}0748013 \qquad c={\cdot}991548 \qquad e={\cdot}0548442$$

Substituting in the preceding equations, and writing the logarithms of the coefficients instead of the coefficients themselves, we get

$$r_1=0{\cdot}1460995\,\mathfrak{r}_1-0{\cdot}2308405\,R_1-8{\cdot}5192440\,R_1'$$

$$r_3=-0{\cdot}4450058\,\mathfrak{r}_3+1{\cdot}2154967\,R_3+9{\cdot}8181930\,R_3'$$

$$r_4 = 0{\cdot}0535010\,\mathrm{r}_4 - 9{\cdot}7596140\,R_4 - 7{\cdot}8675954\,R_4'$$

$$r_5 = -\,7{\cdot}7463524\,\mathrm{r}_5 + 0{\cdot}2995642\,R_5 + 0{\cdot}2995642\,R_5'$$

$$r_6 = 0{\cdot}1617938\,\mathrm{r}_6 - 0{\cdot}2917755\,R_6 - 8{\cdot}5887003\,R'_6$$

$$r_7 = 0{\cdot}1326574\,\mathrm{r}_7 - 0{\cdot}1741219\,R_7 - 8{\cdot}4541703\,R_7'$$

$$r_9 = -\,8{\cdot}2495414\,\mathrm{r}_9 + 0{\cdot}2456727\,R_9 - 0{\cdot}0558873\,R_9'$$

$$r_{10} = 0{\cdot}0267023\,\mathrm{r}_{10} - 9{\cdot}4699640\,R_{10} - 7{\cdot}4508570\,R_{10}'$$

$$r_{11} = 0{\cdot}9148582\,\mathrm{r}_{11} - 1{\cdot}4456131\,R_{11} - 0{\cdot}0060992\,R_{11}'$$

$$r_{12} = 0{\cdot}1990183\,\mathrm{r}_{12} + 1{\cdot}0704790\,R_{12} + 9{\cdot}6909293\,R_{12}'$$

$$r_{13} = 0{\cdot}0504044\,\mathrm{r}_{13} - 9{\cdot}7282013\,R_{13} - 7{\cdot}8306471\,R_{13}'$$

$$r_{14} = 0{\cdot}7176313\,\mathrm{r}_{14} + 1{\cdot}4125573\,R_{14} + 0{\cdot}0058216\,R_{14}'$$

$$r_{15} = -\,0{\cdot}8282531\,\mathrm{r}_{15} + 1{\cdot}5070002\,R_{15} + 0{\cdot}0926384\,R_{15}'$$

$$r_{16} = 0{\cdot}0568761\,\mathrm{r}_{16} - 9{\cdot}7921334\,R_{16} - 7{\cdot}9057198\,R_{16}'$$

$$r_{17} = -\,8{\cdot}3558051\,\mathrm{r}_{17} + 0{\cdot}3069571\,R_{17} + 0{\cdot}3069571\,R_{17}'$$

$$r_{18} = 0{\cdot}1803182\,\mathrm{r}_{18} - 0{\cdot}3576881\,R_{18} - 8{\cdot}6633026\,R_{18}'$$

$$r_{19} = 0{\cdot}1210357\,\mathrm{r}_{19} - 0{\cdot}1210357\,R_{19} - 8{\cdot}3928848\,R_{19}'$$

$$r_{101} = -\,0{\cdot}7701834\,\mathrm{r}_{101} + 1{\cdot}5505062\,R_{101} + 0{\cdot}0464175\,R_{101}'$$

$$r_{102} = -\,7{\cdot}6416818\,\mathrm{r}_{102} + 0{\cdot}4365911\,R_{102} - 0{\cdot}3511177\,R_{102}'$$

$$r_{103} = 0{\cdot}1340779\,\mathrm{r}_{103} - 0{\cdot}2746455\,R_{103} - 8{\cdot}4613229\,R_{103}'$$

$$r_{104} = -\,0{\cdot}4131392\,\mathrm{r}_{104} + 1{\cdot}2823979\,R_{104} + 9{\cdot}7992116\,R_{104}'$$

These quantities introduce into the expression for the longitude expressed in sexagesimal seconds, the terms,

$$+\{5{\cdot}4942896\,\mathrm{r}_1 - 5{\cdot}5790306\,R_1 - 3{\cdot}8674341\,R_1'\}\sin 2t \qquad [4{\cdot}7798951]$$

$$+\{-\,4{\cdot}8656743\,\mathrm{r}_3 + 5{\cdot}6361652\,R_3 + 4{\cdot}2382615\,R_3'\}\sin(2t - x) \qquad [4{\cdot}1857212]$$

$$+\{3{\cdot}9544710\,\mathrm{r}_4 - 3{\cdot}6605840\,R_4 - 1{\cdot}7685654\,R_4'\}\sin(2t + x) \qquad [3{\cdot}1463242]$$

$$+\{-\,2{\cdot}7130189\,\mathrm{r}_5 + 5{\cdot}2662307\,R_5 + 5{\cdot}2662307\,R_5'\}\sin z \qquad [5{\cdot}7917274]$$

$$+\{3{\cdot}7530252\,\mathrm{r}_6 - 3{\cdot}8830069\,R_6 - 2{\cdot}1799317\,R_6'\}\sin(2t - z) \qquad [3{\cdot}0408572]$$

$$+\{3{\cdot}6887576\,\mathrm{r}_7 - 3{\cdot}7302221\,R_7 - 2{\cdot}0102705\,R_7'\}\sin(2t + z) \qquad [2{\cdot}9705948]$$

$$+\{2{\cdot}2203935\,\mathrm{r}_9 - 4{\cdot}2165248\,R_9 + 4{\cdot}0267394\,R_9'\}\sin(2t - 2x) \qquad [4{\cdot}5469577]$$

$$+\{2{\cdot}5368240\,\mathrm{r}_{10} - 1{\cdot}9800857\,R_{10} - 9{\cdot}9609787\,R_{10}'\}\sin(2t + 2x) \qquad [1{\cdot}6254969]$$

$$+\{3{\cdot}4666708\,\mathrm{r}_{11} - 3{\cdot}9974257\,R_{11} - 2{\cdot}5579118\,R_{11}'\}\sin(x + z) \qquad [2{\cdot}2228889]$$

$$+\{-2{\cdot}8843819\,\mathrm{r}_{12}+3{\cdot}7558426\,R_{12}+2{\cdot}3762929\,R_{12}'\}\sin(2t-x-z)\qquad[2{\cdot}4899904]$$

$$+\{2{\cdot}1652119\,\mathrm{r}_{13}-1{\cdot}8430088\,R_{13}-9{\cdot}9454546\,R_{13}'\}\sin(2t+x+z)\qquad[1{\cdot}3488787]$$

$$+\{-3{\cdot}3350850\,\mathrm{r}_{14}+4{\cdot}0300110\,R_{14}+2{\cdot}6232753\,R_{14}'\}\sin(x-z)\qquad[2{\cdot}3541741]$$

$$+\{-3{\cdot}4377718\,\mathrm{r}_{15}+4{\cdot}1169189\,R_{15}+2{\cdot}7021571\,R_{15}'\}\sin(2t-x+z)\qquad[2{\cdot}3383041]$$

$$+\{2{\cdot}1945476\,\mathrm{r}_{16}-1{\cdot}9298049\,R_{16}-0{\cdot}0433913\,R_{16}'\}\sin(2t+x-z)\qquad[1{\cdot}3946097]$$

$$+\{-1{\cdot}2465621\,\mathrm{r}_{17}+3{\cdot}1977141\,R_{17}+3{\cdot}1977141\,R_{17}'\}\sin 2z\qquad[3{\cdot}4147879]$$

$$+\{2{\cdot}0153626\,\mathrm{r}_{18}-2{\cdot}1927325\,R_{18}-0{\cdot}4983470\,R_{18}'\}\sin(2t-2z)\qquad[1{\cdot}3033627]$$

$$+\{1{\cdot}8857018\,\mathrm{r}_{19}-1{\cdot}8857018\,R_{19}-0{\cdot}1575509\,R_{19}'\}\sin(2t+2z)\qquad[1{\cdot}1626061]$$

$$+\{-6{\cdot}4194035\,\mathrm{r}_{101}+7{\cdot}1997263\,R_{101}+5{\cdot}6956376\,R_{101}'\}\sin t\qquad[5{\cdot}3481901]$$

$$+\{3{\cdot}1744332\,\mathrm{r}_{102}-5{\cdot}9693425\,R_{102}+5{\cdot}8838691\,R_{102}'\}\sin(t-x)\qquad[6{\cdot}4098870]$$

$$+\{4{\cdot}2060990\,\mathrm{r}_{103}-4{\cdot}3466666\,R_{103}-2{\cdot}5333440\,R_{103}'\}\sin(t+x)\qquad[3{\cdot}4884264]$$

$$+\{-4{\cdot}3240929\,\mathrm{r}_{104}+5{\cdot}1933516\,R_{104}+3{\cdot}7101653\,R_{104}'\}\sin(t-z)\qquad[3{\cdot}6803018]$$

The preceding expressions serve to show the extent to which the approximation must be carried in the calculation of the quantities r, R, &c.

If we take the term 5·6361652 R_3, since log. $\frac{m_{,}a^3}{\mu a_{,}^3} = 7{\cdot}7464329$, it is evident that in order not to neglect ·01″ in the value of λ, the coefficient of $\frac{m_{,}a^2}{\mu a_{,}^3}\cos(2t-x)$ in the development of δR must be calculated exactly to the fifth place of decimals, but not beyond. The number 4·1857212 is the logarithm of the quantity $\frac{e}{(2-m-c)^2}$, expressed in sexagesimal seconds, and serves to show in like manner how far the approximation must be carried in the calculation of $\frac{\mathrm{d}R}{\mathrm{d}\lambda}$.

When the square of the disturbing force is neglected,

$$R_2=\frac{m_{,}a^3}{2\mu a_{,}^3}\qquad R_8=\frac{m_{,}a^3}{8\mu a_{,}^3}\qquad r_0=-\frac{m_{,}a^3}{2\mu a_{,}^3}\qquad \mathrm{r}_2=3\,r_0\qquad \mathrm{r}_8=3\,r_0\qquad r_2=0$$

$$c^2=1+3\,r_0-\frac{2\,m_{,}a^3}{\mu a_{,}^3}=1-\frac{7\,m_{,}a^3}{2\mu a_{,}^3}$$

The equation of p. 5, line 8, gives $r_8 = 0$.

$$\frac{\mathrm{d}\lambda}{\mathrm{d}t}=\frac{h}{r^2}\left\{1-\frac{1}{h}\int\frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}t+\frac{1}{2h^2}\left\{\int\frac{\mathrm{d}R}{\mathrm{d}\lambda}\mathrm{d}t\right\}^2\right.$$

$$\frac{h}{r^2}=\frac{h(1+s^2)}{r^2}=\frac{h}{a^2}\left\{\frac{a}{\mathrm{r}}+a\,\delta\frac{1}{r}\right\}^2\left\{1+s^2\right\}$$

$$= \frac{h}{a^2}\left\{\frac{a^2}{\mathrm{r}^2} + \frac{2\,a^2}{\mathrm{r}}\,\delta\frac{1}{r} + a^2\left(\delta.\frac{1}{r}\right)^2\right\}\left\{1 + s^2\right\}$$

$$s = \underset{[146]}{\gamma \sin y} + \underset{[147]}{\gamma\, s_{147} \sin(2\,t - y)} \text{ nearly}$$

$$s^2 = \frac{\gamma^2}{2} + \frac{\gamma^2 s^2{}_{147}}{2} - \underset{(1)}{\gamma^2 s_{147} \cos 2\,t} - \underset{(62)}{\frac{\gamma^2}{2}\cos 2\,y} + \underset{(63)}{\gamma^2 s_{147} \cos(2\,t - 2\,y)}$$

$$1 + s^2 = 1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\left\{1 - \underset{[1]}{\gamma^2 s_{147} \cos 2\,t} - \underset{[62]}{\frac{\gamma^2}{2}\cos 2\,y} + \underset{[63]}{\gamma^2 s^2{}_{147} \cos(2\,t - 2\,y)}\right\}$$

nearly

$$\frac{a^2}{\mathrm{r}^2} = 1 + \frac{e^2}{2}\left(1 + \frac{3}{4}e^2\right) + \underset{[2]}{2\,e\left(1 + \frac{3}{8}e^2\right)\cos x} + \underset{[8]}{\frac{5}{2}e^2\left(1 + \frac{2}{15}e^2\right)\cos 2\,x}$$

$$+ \underset{[20]}{\frac{13}{4}e^3 \cos 3\,x} + \underset{[38]}{\frac{103}{24}e^4 \cos 4\,x}$$

$$\frac{a}{\mathrm{r}} = 1 + \underset{[2]}{e\left(1 - \frac{e^2}{8}\right)\cos x} + \underset{[8]}{e^2\left(1 - \frac{e^2}{3}\right)\cos 2\,x} + \underset{[20]}{\frac{9}{8}e^3 \cos 3\,x} + \underset{[38]}{\frac{4}{3}e^4 \cos 4\,x}$$

If the coefficients corresponding to the different arguments in the quantity $\frac{a^2}{r^2}$, be called $2\,\mathfrak{r}'_n$ and the coefficients of the different arguments in the development of the quantity

$$-n\,a\left\{\int\frac{\mathrm{d}R}{\mathrm{d}\lambda'}\,\mathrm{d}t - \frac{1}{2\,h^2}\left\{\int\frac{\mathrm{d}R}{\mathrm{d}\lambda'}\,\mathrm{d}t^2\right\}^2\right\} \text{ be called } \mathfrak{R}_n\text{, then}$$

$$2\,\mathrm{r}'_0 = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2}s^2{}_{147}{}^*\right\}\left\{1 + \frac{e^2}{2}\left(1 + \frac{3}{4}e^2\right) + 2\,r_0 + r_0{}^2 + \frac{r_1{}^2}{2} + \frac{e^2 r_2{}^2}{2} + \frac{e^2 r_3{}^2}{2}\,\frac{e^2 r_4{}^2}{2} + \frac{e_{\prime}{}^2 r_5{}^2}{2}\right.$$

$$\left. + \frac{e_{\prime}{}^2 r_6{}^2}{2} + \frac{e_{\prime}{}^2 r_7{}^2}{2}\right\}$$

$$\mathrm{r}'_1 = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2}s^2{}_{147}\right\}\left\{r_1 - \gamma^2 s_{147} + \frac{e^2}{2}\left(1 - \frac{e^2}{8}\right)\left\{r_3 + r_4\right\} + \frac{e^4}{2}\left\{r_9 + r_{10}\right\} + 2\,r_0 r_1\right.$$

$$\left. + e^2(r_3 + r_4)\,r_2 + e_{\prime}{}^2(r_6 + r_7)\,r_5\right\}$$

$$\mathrm{r}'_2 = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2}s^2{}_{147}\right\}\left\{1 + \frac{3}{8}e^2 + r_2 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{2\,r_0 + e^2 r_8\right\} + \frac{e^2}{2}r_2\right.$$

$$\left. + (r_4 + r_3)\,r_1 + 2\,r_0 r_2\right\}$$

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* $(s_{147})^2$ is intended.

$$\mathrm{r}_3' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_3 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_9 + r_1 - \gamma^2 s_{147}\right\} + \frac{e^2}{2} r_4 + r_1 r_2 + 2 r_0 r_3\right\}$$

$$\mathrm{r}_4' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_4 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{r_1 - \gamma^2 s_{147} + e^2 r_{10}\right\} + \frac{e^2}{2} r_3 + r_1 r_2 + 2 r_0 r_4\right\}$$

$$\mathrm{r}_5' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_5 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{14} + e^2 r_{11}\right\} + r_1 r_7 + r_1 r_6 + 2 r_0 r_5\right\}$$

$$\mathrm{r}_6' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_6 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{12} + e^2 r_{16}\right\} + r_5 r_1 + 2 r_0 r_6\right\}$$

$$\mathrm{r}_7' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_7 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{15} + e^2 r_{13}\right\} + r_5 r_1 + 2 r_0 r_7\right\}$$

$$\mathrm{r}_8' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_8 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{r_2 + e^2 r_{20}\right\} + r_0 + \frac{9}{16} e^2 r_2\right.$$

$$\left. + r_2^2 + r_4 r_3 + r_1 r_9 + r_1 r_{10}\right\}$$

$$\mathrm{r}_9' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_9 + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{21} + r_3\right\} + \frac{r_1}{2} - \frac{\gamma^2}{2} s_{147}\right.$$

$$\left. + \frac{9}{16} e^2 r_4 + r_2 r_3 + 2 r_0 r_9\right\}$$

$$\mathrm{r}_{10}' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{10} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{r_4 + e^2 r_{22}\right\} + \frac{r_1}{2} - \frac{\gamma^2}{2} s_{147}\right.$$

$$\left. + \frac{9}{16} e^2 r_3 + r_4 r_2 + 2 r_0 r_{10}\right\}$$

$$\mathrm{r}_{11}' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{11} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{r_5 + e^2 r_{23}\right\} + \frac{e^2}{2} r_{14} + r_1 r_{13} + r_1 r_{12} + r_2 r_5\right.$$

$$\left. + r_6 r_4 + r_3 r_7 + 2 r_0 r_{11}\right\}$$

$$\mathrm{r}_{12}' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{12} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{24} + r_6\right\} + \frac{e^2}{2} r_{16} + r_{11} r_1 + r_2 r_6 + r_5 r_3 + 2 r_0 r_{12}\right\}$$

$$\mathrm{r}_{13}' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{13} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{r_7 + e^2 r_{25}\right\} + \frac{e^2}{2} r_{15} + r_{11} r_1 + r_2 r_7 + r_3 r_4 + 2 r_0 r_{13}\right\}$$

$$\mathrm{r}_{14}' = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{14} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right) \left\{e^2 r_{26} + r_5\right\} + \frac{e^2}{2} r_{11} + r_{16} r_1 + r_{15} r_1 + r_2 r_5 + r_6 r_3\right.$$

$$\left. + r_7 r_4 + 2 r_0 r_{14}\right\}$$

$$\mathfrak{r}_{15}^{\backprime} = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{15} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{e^2 r_{27} + r_7\right\} + \frac{e^2}{2} r_{13} + r_{14} r_1 + r_2 r_7 + r_5 r_3\right\}$$

$$\mathfrak{r}_{16}^{\backprime} = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{16} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{r_6 + e^2 r_{28}\right\} + \frac{e^2}{2} r_{12} + r_{14} r_1 + r_2 r_6 + r_5 r_4\right\}$$

$$\mathfrak{r}_{17}^{\backprime} = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{17} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{e^2 r_{32} + e^2 r_{29}\right\} + r_5^2 + r_7 r_6 + r_1 r_{18} + r_1 r_{19}\right\}$$

$$\mathfrak{r}_{18}^{\backprime} = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{18} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{e^2 r_{30} + e^2 r_{34}\right\} + r_{17} r_1 + r_5 r_6\right\}$$

$$\mathfrak{r}_{19}^{\backprime} = \left\{1 + \frac{\gamma^2}{2} + \frac{\gamma^2}{2} s^2{}_{147}\right\} \left\{r_{19} + \frac{1}{2}\left(1 - \frac{e^2}{8}\right)\left\{e^2 r_{33} + e^2 r_{31}\right\} + r_{17} r_1 + r_7 r_5\right\}$$

$$\lambda^{\backprime} = \left\{\frac{2 h \mathfrak{r}_0}{a^2} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_1 + e^2\mathfrak{r}_3^{\backprime}\mathfrak{R}_3 + e^2\mathfrak{r}_4^{\backprime}\mathfrak{R}_4 + e_{\prime}^2\mathfrak{r}_5^{\backprime}\mathfrak{R}_5 + e_{\prime}^2\mathfrak{r}_6^{\backprime}\mathfrak{R}_6 + e_{\prime}^2\mathfrak{r}_7^{\backprime}\mathfrak{R}_7\right\} t$$

$$+ \frac{1}{2 - 2m}\{2\mathfrak{r}_1^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_1 + 2\mathfrak{r}_1^{\backprime}\mathfrak{R}_0 + e^2\mathfrak{r}_2^{\backprime}\mathfrak{R}_3 + e^2\mathfrak{r}_2^{\backprime}\mathfrak{R}_4 + e^2\mathfrak{r}_3^{\backprime}\mathfrak{R}_2 + e^2\mathfrak{r}_4\,\mathfrak{R}_2$$

$$+ e_{\prime}^2\mathfrak{r}_5^{\backprime}\mathfrak{R}_6 + e_{\prime}^2\mathfrak{r}_5^{\backprime}\mathfrak{R}_7 + e_{\prime}^2\mathfrak{r}_6^{\backprime}\mathfrak{R}_5 + e_{\prime}^2\mathfrak{r}_7^{\backprime}\mathfrak{R}_5\}\sin 2t$$

[1]

$$+ \frac{1}{c}\{2\mathfrak{r}_2^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_2 + 2\mathfrak{r}_2^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_4 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_3 + \mathfrak{r}_3^{\backprime}\mathfrak{R}_1 + \mathfrak{r}_4^{\backprime}\mathfrak{R}_1\}\, e\sin x$$

[2]

$$+ \frac{1}{(2 - 2m - c)}\{2\mathfrak{r}_3^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_3 + 2\mathfrak{r}_3^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_2 + \mathfrak{r}_2^{\backprime}\mathfrak{R}_1\}\, e\sin(2t - x)$$

(3)

$$+ \frac{1}{(2 - 2m + c)}\{2\mathfrak{r}_4^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_4 + 2\mathfrak{r}_4^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_2 + \mathfrak{r}_2^{\backprime}\mathfrak{R}_1\}\, e\sin(2t + x)$$

[4]

$$+ \frac{1}{m}\{2\mathfrak{r}_5^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_5 + 2\mathfrak{r}_5^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_7 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_6 + \mathfrak{r}_6^{\backprime}\mathfrak{R}_1 + \mathfrak{r}_7^{\backprime}\mathfrak{R}_1\}\, e_{\prime}\sin z$$

[5]

$$+ \frac{1}{(2 - 3m)}\{2\mathfrak{r}_6^{\backprime} + 2\mathfrak{r}_0\,\mathfrak{R}_6 + 2\mathfrak{r}_6^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1\,\mathfrak{R}_5 + \mathfrak{r}_5^{\backprime}\mathfrak{R}_1\}\, e_{\prime}\sin(2t - z)$$

[6]

$$+ \frac{1}{(2 - m)}\{2\mathfrak{r}_7^{\backprime} + 2\mathfrak{r}_0^{\backprime}\mathfrak{R}_7 + 2\mathfrak{r}_7^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_5 + \mathfrak{r}_5^{\backprime}\mathfrak{R}_1\}\, e_{\prime}\sin(2t + z)$$

[7]

$$+ \frac{1}{2c}\{2\mathfrak{r}_8^{\backprime} + 2\mathfrak{r}_0\,\mathfrak{R}_8 + 2\mathfrak{r}_8^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_{10} + \mathfrak{r}_1^{\backprime}\mathfrak{R}_9 + \mathfrak{r}_2^{\backprime}\mathfrak{R}_2 + \mathfrak{r}_3^{\backprime}\mathfrak{R}_4 + \mathfrak{r}_4^{\backprime}\mathfrak{R}_3 + \mathfrak{r}_9^{\backprime}\mathfrak{R}_1 + \mathfrak{r}_{10}^{\backprime}\mathfrak{R}_1\}\, e^2\sin 2x$$

[8]

$$+ \frac{1}{(2 - 2m - 2c)}\{2\mathfrak{r}_9^{\backprime} + 2\mathfrak{r}_0\,\mathfrak{R}_9 + 2\mathfrak{r}_9^{\backprime}\mathfrak{R}_0 + \mathfrak{r}_1^{\backprime}\mathfrak{R}_8 + \mathfrak{r}_2^{\backprime}\mathfrak{R}_3 + \mathfrak{r}_3'\mathfrak{R}_2 + \mathfrak{r}_8^{\backprime}\mathfrak{R}_1\}\, e^2\sin(2t - 2x)$$

[9]

$$+\frac{1}{(2-2m+2c)}\left\{2\mathfrak{r}_{10}^{\backprime}+2\mathfrak{r}_{0}\,\mathfrak{R}_{10}+2\mathfrak{r}_{10}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{8}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{4}+\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{2}+\mathfrak{r}_{8}^{\backprime}\mathfrak{R}_{1}\right\}e^{2}\sin(2t+2x)$$

[10]

$$+\frac{1}{(c+m)}\left\{2\mathfrak{r}_{11}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{11}+2\mathfrak{r}_{11}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{13}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{12}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{3}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{2}\right.$$

$$\left.+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{4}+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{3}+\mathfrak{r}_{\cdot 2}^{\backprime}\mathfrak{R}_{1}+\mathfrak{r}_{13}^{\backprime}\mathfrak{R}_{1}\right\}e\,e_{,}\sin(x+z)$$

[11]

$$+\frac{1}{(2-3m-c)}\left\{2\mathfrak{r}_{12}^{\backprime}+2\mathfrak{r}_{0}\,\mathfrak{R}_{12}+2\mathfrak{r}_{12}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{11}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{3}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{3}+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{2}\right.$$

$$\left.+\mathfrak{r}_{11}\,\mathfrak{R}_{1}\right\}e\,e_{,}\sin(2t-x-z)$$

[12]

$$+\frac{1}{(2-m+c)}\left\{2\mathfrak{r}_{13}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{13}+2\mathfrak{r}_{13}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}\,\mathfrak{R}_{11}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{4}\right.$$

$$\left.+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{2}+\mathfrak{r}_{11}^{\backprime}\mathfrak{R}_{1}\right\}e\,e_{,}\sin(2t+x+z)$$

[13]

$$+\frac{1}{(c-m)}\left\{2\mathfrak{r}_{14}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{14}+2\mathfrak{r}_{14}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{16}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{15}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{3}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{5}\,\mathfrak{R}_{2}\right.$$

$$\left.+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{3}+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{4}+\mathfrak{r}_{15}^{\backprime}\mathfrak{R}_{1}+\mathfrak{r}_{16}^{\backprime}\mathfrak{R}_{1}\right\}e\,e_{,}\sin(x-z)$$

[14]

$$+\frac{1}{(2-m-c)}\left\{2\mathfrak{r}_{15}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{15}+2\mathfrak{r}_{15}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{14}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{3}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{3}+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{2}\right.$$

$$\left.+\mathfrak{r}_{14}^{\backprime}\mathfrak{R}_{1}\right\}e\,e_{,}\sin(2t-x+z)$$

[15]

$$+\frac{1}{(2-3m+c)}\left\{2\mathfrak{r}_{16}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{16}+2\mathfrak{r}_{16}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{14}+\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{4}+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{2}\right.$$

$$\left.+\mathfrak{r}_{14}^{\backprime}\mathfrak{R}_{1}\right\}e\,e_{,}\sin(2t+x+z)$$

[16]

$$+\frac{1}{2m}\left\{2\mathfrak{r}_{17}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{17}+2\mathfrak{r}_{17}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{19}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{18}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{18}^{\backprime}\mathfrak{R}_{1}\right.$$

$$\left.+\mathfrak{r}_{19}^{\backprime}\mathfrak{R}_{1}\right\}e_{,}^{2}\sin 2z$$

[17]

$$+\frac{1}{(2-4m)}\left\{2\mathfrak{r}_{18}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{18}+2\mathfrak{r}_{18}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{17}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{6}+\mathfrak{r}_{6}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{17}^{\backprime}\mathfrak{R}_{1}\right\}e_{,}^{2}\sin(2t-2z)$$

[18]

$$+\frac{1}{2}\left\{2\mathfrak{r}_{19}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{19}+2\mathfrak{r}_{19}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{17}+\mathfrak{r}_{5}^{\backprime}\mathfrak{R}_{7}+\mathfrak{r}_{7}^{\backprime}\mathfrak{R}_{5}+\mathfrak{r}_{17}^{\backprime}\mathfrak{R}_{1}\right\}e_{,}^{2}\sin(2t+2z)$$

[19]

$$+\frac{1}{1-m}\left\{2\mathfrak{r}_{101}^{\backprime}+2\mathfrak{r}_{0}^{\backprime}\mathfrak{R}_{101}+2\mathfrak{r}_{101}^{\backprime}\mathfrak{R}_{0}+\mathfrak{r}_{1}^{\backprime}\mathfrak{R}_{101}^{\backprime}+e^{2}\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{102}+e^{2}\mathfrak{r}_{2}^{\backprime}\mathfrak{R}_{103}+e^{2}\mathfrak{r}_{3}\,\mathfrak{R}_{102}\right.$$

$$\left.+e^{2}\mathfrak{r}_{4}^{\backprime}\mathfrak{R}_{103}+e_{,}^{2}r_{5}\mathfrak{R}_{104}+e_{,}^{2}r_{5}\mathfrak{R}_{105}+r_{101}\mathfrak{R}_{1}\right\}\sin t$$

[101]

These examples will serve for the present to show how the development may be obtained from Table II.

M. DAMOISEAU has given (Mém. sur la Théorie de la Lune, p. 348,) the expression for $a\,\delta\,\frac{1}{r}$ in terms of the true longitude. In order to obtain a comparison of his results with those which may be obtained by the preceding method, it is necessary to transform his expressions, which may be done by LAGRANGE's theorem, into series containing explicitly the mean longitude.

If we suppose

$$\frac{a}{r'} = A_0 + A_1 \cos(2\lambda' - 2m\lambda') + e A_2 \cos(c\lambda' - \varpi) + e A_3 \cos(2\lambda' - 2m\lambda' - c\lambda' + \varpi) + \&c.$$

$$s = B_{146}\gamma \sin(g\lambda' - \nu) + B_{147}\gamma \sin(2\lambda' - 2m\lambda - g\lambda' + \nu) + B_{148}\gamma \sin(2\lambda' - 2m\lambda' + g\lambda' - \nu) + \&c.$$

$$n t = \lambda' + C_1 \sin(2\lambda' - 2m\lambda') + e C_2 \sin(c\lambda' - \varpi) + e C_3 \sin(2\lambda - 2m\lambda' - c\lambda' + \varpi) + \&c.$$

in which expressions A, B, C are the same quantities as in M. DAMOISEAU's notation, the indices only being changed according to the remark, Phil. Trans. 1830, p. 246, in order that Table II. may be applicable to the transformation required; λ' is called v, and $\delta\,.\,\frac{1}{r'}$, δu in the notation of M. DAMOISEAU.

$$\frac{a}{r'} = A_0 + \frac{1}{2}(2-2m) A_1 C_1 + \frac{c}{2} e^2 A_2 C_2 + \frac{1}{2}(2-2m-c) e^2 A_3 C_3 + \frac{1}{2}(2-2m+c) e^2 A_4 C_4$$

$$+ \frac{m}{2} e_i^2 A_5 C_5 + \&c.$$

$$+ \left\{ A_1 - \frac{1}{2} c e^2 A_2 C_3 + \frac{1}{2} c e^2 A_2 C_4 - \frac{1}{2}(2-2m-c) e^2 A_3 C_2 + \frac{1}{2}(2-2m+c) e^2 A_4 C_2 \right.$$

$$\left. - \frac{1}{2} m e_i^2 A_5 C_6 + \frac{1}{2} m e_i^2 A_5 C_7 - \frac{1}{2}(2-3m) e_i^2 A_6 C_5 + \frac{1}{2}(2-m) e_i^2 A_7 C_5 \right\} \cos 2t$$

[1]

$$+ \left\{ A_2 + \frac{1}{2}(2-2m) A_1 C_4 + \frac{1}{2}(2-2m) A_1 C_3 + \frac{1}{2}(2-2m-c) A_3 C_1 \right.$$

$$\left. + \frac{1}{2}(2-2m+c) A_4 C_1 \right\} e \cos x$$

[2]

$$+ \left\{ A_3 + \frac{1}{2}(2-2m) A_1 C_2 + \frac{c}{2} A_2 C_1 \right\} e \cos(2t - x)$$

[3]

$$+ \left\{ A_4 - \frac{1}{2}(2-2m) A_1 C_2 - \frac{c}{2} A_2 C_1 \right\} e \cos(2t + x)$$

[4]

$$
\begin{aligned}
&+\Big\{A_5+\frac{1}{2}(2-2m)A_1C_7+\frac{1}{2}(2-2m)A_1C_6+\frac{1}{2}(2-3m)A_6C_1\\
&\qquad+\frac{1}{2}(2-m)A_7C_1\Big\}e_{,}\cos z
\end{aligned}
$$

[5]

$$
+\Big\{A_6+\frac{1}{2}(2-2m)A_1C_5+\frac{m}{2}A_5C_1\Big\}e_{,}\cos(2t-z)
$$

[6]

$$
+\Big\{A_7-\frac{1}{2}(2-2m)A_1C_5-\frac{m}{2}A_5C_1\Big\}e_{,}\cos(2t+z)
$$

[7]

$$
\begin{aligned}
&+\Big\{A_8+\frac{1}{2}(2-2m)A_1C_{10}+\frac{1}{2}(2-2m)A_1C_9-\frac{c}{2}A_2C_2+\frac{1}{2}(2-2m-c)A_3C_4\\
&\qquad+\frac{1}{2}(2-2m+c)A_4C_3+\frac{1}{2}(2-2m-2c)A_9C_1+\frac{1}{2}(2-2m+2c)A_{10}C_1\Big\}e^2\cos 2x
\end{aligned}
$$

[8]

$$
+\Big\{A_9+\frac{1}{2}(2-2m)A_1C_8+\frac{c}{2}A_2C_3+\frac{1}{2}(2-2m-c)A_3C_2+cA_8C_1\Big\}e^2\cos(2t-2x)
$$

[9]

$$
+\Big\{A_{10}-\frac{1}{2}(2-2m)A_1C_8-\frac{c}{2}A_2C_4-\frac{1}{2}(2-2m+c)A_4C_2-cA_8C_1\Big\}e^2\cos(2t+2x)
$$

[10]

$$
\begin{aligned}
&+\Big\{A_{11}+\frac{1}{2}(2-2m)A_1C_{13}+\frac{1}{2}(2-2m)A_1C_{12}-\frac{c}{2}A_2C_5+\frac{1}{2}(2-2m-c)A_3C_7\\
&\qquad+\frac{1}{2}(2-2m+c)A_4C_6-\frac{m}{2}A_5C_2+\frac{1}{2}(2-3m)A_6C_4+\frac{1}{2}(2-m)A_7C_3\\
&\qquad+\frac{1}{2}(2-3m-c)A_{12}C_1\Big\}ee_{,}\cos(x+z)
\end{aligned}
$$

[11]

$$
\begin{aligned}
&+\Big\{A_{12}+\frac{1}{2}(2-2m)A_1C_{11}+\frac{c}{2}A_2C_6+\frac{1}{2}(2-2m-c)A_3C_5+\frac{m}{2}A_5C_3\\
&\qquad+\frac{1}{2}(2-3m)A_6C_2+\frac{1}{2}(c+m)A_{11}C_1\Big\}ee_{,}\cos(2t-x-z)
\end{aligned}
$$

[12]

$$
\begin{aligned}
&+\Big\{A_{13}-\frac{1}{2}(2-2m)A_1C_{11}-\frac{c}{2}A_2C_7-\frac{1}{2}(2-2m+c)A_4C_5-\frac{m}{2}A_5C_4\\
&\qquad-\frac{1}{2}(2-m)A_7C_2-\frac{1}{2}(c+m)A_{11}C_1\Big\}ee_{,}\cos(2t+x+z)
\end{aligned}
$$

[13]

$$
\begin{aligned}
&+\Big\{A_{14}+\frac{1}{2}(2-2m)A_1C_{16}+\frac{1}{2}(2-2m)A_1C_{15}+\frac{c}{2}A_2C_5+\frac{1}{2}(2-2m-c)A_3C_6\\
&\qquad+\frac{1}{2}(2-2m+c)A_4C_7+\frac{m}{2}A_5C_2+\frac{1}{2}(2-3m)A_6C_3+\frac{1}{2}(2-m)A_7C_4
\end{aligned}
$$

$$+\frac{1}{2}(2-3m+c)A_{15}C_1+\frac{1}{2}(2-m-c)A_{16}C_1\Big\}ee_{,}\cos(x-z)$$

[14]

$$+\Big\{A_{15}+\frac{1}{2}(2-2m)A_1C_{14}+\frac{c}{2}A_2C_7-\frac{1}{2}(2-2m-c)A_3C_5-\frac{m}{2}A_5C_3$$

$$+\frac{1}{2}(2-m)A_7C_2+\frac{1}{2}(c-m)A_{14}C_1\Big\}ee_{,}\cos(2t-x+z)$$

[15]

$$+\Big\{A_{16}-\frac{1}{2}(2-2m)A_1C_{14}-\frac{c}{2}A_2C_6+\frac{1}{2}(2-2m+c)A_4C_5+\frac{m}{2}A_5C_4$$

$$-\frac{1}{2}(2-3m)A_6C_2-\frac{1}{2}(c-m)A_{14}C_1\Big\}ee_{,}\cos(2t+x-z)$$

[16]

$$+\Big\{A_{17}+\frac{1}{2}(2-2m)A_1C_{19}+\frac{1}{2}(2-2m)A_1C_{18}-\frac{m}{2}A_5C_5+\frac{1}{2}(2-3m)A_6C_7$$

$$+\frac{1}{2}(2-m)A_7C_6+\frac{1}{2}(2-4m)A_{18}C_1+A_{19}C_1\Big\}e_{,}^2\cos 2z$$

[17]

$$+\Big\{A_{18}+\frac{1}{2}(2-2m)A_1C_{17}+\frac{m}{2}A_5C_6+\frac{1}{2}(2-3m)A_6C_5+mA_{17}C_1\Big\}e_{,}^2\cos(2t-2z)$$

[18]

$$+\Big\{A_{19}-\frac{1}{2}(2-2m)A_1C_{17}-\frac{m}{2}A_5C_7-\frac{1}{2}(2-m)A_6C_5-mA_{17}C_1\Big\}e_{,}^2\cos(2t+2z)$$

[19]

Similarly

$$s=\Big\{B_{146}+\frac{1}{2}(2-2m+g)C_1B_{148}-\frac{1}{2}(2-2m-g)C_1B_{147}+\frac{1}{2}(c+g)e^2C_2B_{150}$$

$$-\frac{1}{2}(c-g)e^2C_2B_{149}+\frac{1}{2}(2-2m-c+g)e^2C_3B_{152}-\frac{1}{2}(2-2m-c-g)e^2C_3B_{151}$$

$$+\frac{1}{2}(2-2m+c+g)C_4B_{154}-\frac{1}{2}(2-2m+c-g)C_4B_{153}$$

$$+\frac{1}{2}(m+g)e_{,}^2C_5B_{156}-\frac{1}{2}(m-g)e_{,}^2C_5B_{155}\Big\}\gamma\sin y$$

[146]

$$+\Big\{B_{147}-\frac{g}{2}C_1B_{146}-\frac{1}{2}(2-2m-c-g)e^2C_2B_{151}+\frac{1}{2}(2-2m+c-g)e^2C_2B_{153}$$

$$-\frac{1}{2}(c-g)e^2C_3B_{149}-\frac{1}{2}(c+g)C_4B_{150}-\frac{1}{2}(2-3m-g)e_{,}^2C_5B_{157}$$

$$+\frac{1}{2}(2-m-g)e_{,}^2C_5B_{159}\Big\}\gamma\sin(2t-y)$$

[147]

$$+\left\{B_{148}-\frac{g}{2}\,C_1\,B_{146}-\frac{1}{2}\,(2-2m-c+g)\,e^2\,C_2\,B_{152}+\frac{1}{2}\,(2-2m+c+g)\,e^2\,C_2\,B_{154}\right.$$

$$-\frac{1}{2}\,(c+g)\,e^2\,C_3\,B_{150}-\frac{1}{2}\,(c-g)\,C_4\,B_{149}-\frac{1}{2}\,(2-3m+g)\,e_{,}^2\,C_5\,B_{158}$$

$$\left.+\frac{1}{2}\,(2-m+g)\,e_{,}^2\,C_5\,B_{160}\right\}\gamma\sin(2t+y)$$

[148]

$$+\left\{B_{149}+\frac{1}{2}\,(2-2m+c-g)\,C_1\,B_{153}-\frac{1}{2}\,(2-2m-c+g)\,C_1\,B_{152}-\frac{g}{2}\,C_2\,B_{146}\right.$$

$$\left.+\frac{1}{2}\,(2-2m-g)\,C_3\,B_{147}-\frac{1}{2}\,(2-2m+g)\,C_4\,B_{148}\right\}e\gamma\sin(x-y)$$

[149]

$$+\left\{B_{150}+\frac{1}{2}\,(2-2m+c+g)\,C_1\,B_{154}-\frac{1}{2}\,(2-2m-c-g)\,C_1\,B_{151}-\frac{g}{2}\,C_2\,B_{146}\right.$$

$$\left.+\frac{1}{2}\,(2-2m+g)\,C_3\,B_{148}-\frac{1}{2}\,(2-2m-g)\,C_4\,B_{147}\right\}e\gamma\sin(x+y)$$

[150]

$$+\left\{B_{151}-\frac{1}{2}\,(c+g)\,C_1\,B_{150}+\frac{1}{2}\,(2-2m-g)\,C_2\,B_{147}-\frac{g}{2}\,C_3\,B_{146}\right\}e\gamma\sin(2t-x-y)$$

[151]

$$+\left\{B_{152}-\frac{1}{2}\,(c-g)\,C_1\,B_{149}+\frac{1}{2}\,(2-2m+g)\,C_2\,B_{148}-\frac{g}{2}\,C_3\,B_{146}\right\}e\gamma\sin(2t-x+y)$$

[152]

$$+\left\{B_{153}-\frac{1}{2}\,(c-g)\,C_1\,B_{149}-\frac{1}{2}\,(2-2m-g)\,C_2\,B_{147}-\frac{g}{2}\,C_4\,B_{146}\right\}e\gamma\sin(2t+x-y)$$

[153]

$$+\left\{B_{154}-\frac{1}{2}\,(c+g)\,C_1\,B_{150}-\frac{1}{2}\,(2-2m+g)\,C_2\,B_{148}-\frac{g}{2}\,C_4\,B_{146}\right\}e\gamma\sin(2t+x+y)$$

[154]

$$+\left\{B_{155}+\frac{1}{2}\,(2-m-g)\,C_1\,B_{159}-\frac{1}{2}\,(2-3m+g)\,C_1\,B_{158}-\frac{g}{2}\,C_5\,B_{146}\right\}e_{,}\gamma\sin(z-y)$$

[155]

$$+\left\{B_{156}+\frac{1}{2}\,(2-m+g)\,C_1\,B_{160}-\frac{1}{2}\,(2-3m-g)\,C_1\,B_{157}-\frac{g}{2}\,C_5\,B_{146}\right\}e_{,}\gamma\sin(z+y)$$

[156]

$$+\left\{B_{157}-\frac{1}{2}\,(m+g)\,C_1\,B_{156}+\frac{1}{2}\,(2-2m-g)\,C_5\,B_{147}\right\}e_{,}\gamma\sin(2t-z-y)$$

[157]

$$+\left\{B_{158}-\frac{1}{2}\,(m-g)\,C_1\,B_{155}+\frac{1}{2}\,(2-2m+g)\,C_5\,B_{148}\right\}e_{,}\gamma\sin(2t-z+y)$$

[158]

$$+\left\{B_{159}-\frac{1}{2}\,(m-g)\,C_1\,B_{155}-\frac{1}{2}\,(2-2m-g)\,C_5\,B_{147}\right\}e_{,}\gamma\sin(2t+z-y)$$

[159]

$$+\left\{B_{160} - \frac{1}{2}(m+g)\,C_1\,B_{156} - \frac{1}{2}\,(2-2m+g)\,C_5\,B_{148}\right\} e_{\prime}\,\gamma \sin(2t+z+y)$$
[160]

In order to verify these expressions, suppose

$$\frac{a}{r'} = A_2\,e \cos(c\lambda' - \varpi) \qquad s = \gamma\,B_{146} \sin(g\lambda' - \nu) \qquad n\,t = \lambda' + C_1 \sin(2\lambda' - 2\,m\,\lambda')$$

Then by **Lagrange**'s theorem, neglecting A^3, $A^2\,C$, &c.

$$\frac{a}{r} = A_2\,e \cos x + c\,e\,A_2\,C_1 \sin 2\,t \sin x \quad \text{nearly}$$

$$= A_2\,e \cos x + \frac{c\,A_2\,C_1}{2}\,e \cos(2\,t - x) - \frac{c\,A_2\,C_1}{2}\,e \cos(2\,t + x)$$
[2] [3] [4]

which terms are found in the expression which I have given above.

Again, by **Lagrange**'s theorem,

$$s = \gamma\,B_{146} \sin y - g\,\gamma\,C_1\,B_{146} \sin 2\,t \cos y$$

$$= \gamma\,B_{146} \sin y - \frac{g\,C_1\,B_{146}}{2}\,\gamma \sin(2\,t - y) - \frac{g\,C_1\,B_{146}\,\gamma}{2} \sin(2\,t + y)$$
[146] [147] [148]

which terms are found in the expression which I have given above.

The numerical values of the quantities A, B, C, according to M. **Damoiseau**, are

$A_0 =$? [30] $A_1 = {\cdot}00709538$ [1] $A_2 =$?
*[31] $A_3 = {\cdot}2024622$ [32] $A_4 = -{\cdot}00369361$ [16] $A_5 = -{\cdot}0056375$
[33] $A_6 = {\cdot}0289158$ [34] $A_7 = -{\cdot}0030859$ [2] $A_8 = {\cdot}003183$?
[35] $A_9 = {\cdot}347942$ [36] $A_{10} = {\cdot}001970$ [19] $A_{11} = -{\cdot}19737$
[41] $A_{12} = {\cdot}516174$ [42] $A_{13} = {\cdot}0026238$ [18] $A_{14} = -{\cdot}286046$
[39] $A_{15} = -{\cdot}060625$ [40] $A_{16} = -{\cdot}014546$ [17] $A_{17} = -{\cdot}006930$
[43] $A_{18} = {\cdot}08125$

[30] $C_1 = -{\cdot}009216$ [1] $C_2 = -2{\cdot}0044055$ [31] $C_3 = -{\cdot}4138664$
[32] $C_4 = {\cdot}012939$ [16] $C_5 = -{\cdot}194385$ [33] $C_6 = -{\cdot}394172$
[34] $C_7 = {\cdot}0038267$ [2] $C_8 = {\cdot}745169$ [35] $C_9 = -{\cdot}286413$
[36] $C_{10} = -{\cdot}012575$ [19] $C_{11} = {\cdot}365516$ [41] $C_{12} = -1{\cdot}08891$
[42] $C_{13} = -{\cdot}008551$ [18] $C_{14} = -{\cdot}607534$ [39] $C_{15} = {\cdot}11587$
[40] $C_{16} = {\cdot}055936$ [17] $C_{17} = {\cdot}12755$ [43] $C_{18} = -{\cdot}11432$

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* These are the indices of the arguments in M. **Damoiseau**'s work.

[0] $B_{147} = \cdot0284942$ [2] $B_{149} = -\cdot019169$ [6] $B_{151} = -\cdot020788$
[5] $B_{153} = \cdot006113$ [8] $B_{155} = -\cdot081170$ [11] $B_{157} = \cdot071237$
[10] $B_{159} = -\cdot0033394$

Having found the coefficients of $\frac{a}{r^{\backprime}}$, those of $\frac{a}{r}$ are easily determined.

$$\frac{a}{r} = \frac{a}{r^{\backprime}(1+s^2)} = \frac{a}{r^{\backprime}}\left\{1 - \frac{s^2}{2}\right\}$$

$$= \frac{a}{r^{\backprime}}\left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147} + \frac{\gamma^2}{2}s_{147}\cos 2t\right.$$

$$\left. + \frac{\gamma^2}{4}\cos 2y - \frac{\gamma^2}{2}s_{147}\cos(2t - 2y)\right.$$

If the coefficients of $\frac{a}{r^{\backprime}}$ be called $r_n^{\backprime}$,

$$r_0 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_0 + \frac{\gamma^2}{4}s_{147}\, r_1^{\backprime}$$

$$r_1 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_1^{\backprime}$$

$$r_2 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_2^{\backprime} + \frac{\gamma^2}{4}s_{147}\, r_3^{\backprime} + \frac{\gamma^2}{4}s_{147}\, r_4$$

$$r_3 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_3^{\backprime} + \left(1 - \frac{e^2}{8}\right)\frac{\gamma^2}{4}s_{147}$$

$$r_4 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_4^{\backprime} + \left(1 - \frac{e^2}{8}\right)\frac{\gamma^2}{4}s_{147}$$

$$r_5 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_5^{\backprime}$$

$$r_6 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_6^{\backprime} + \frac{\gamma^2}{4}s_{147}\, r_5^{\backprime}$$

$$r_7 = \left\{1 - \frac{\gamma^2}{4} - \frac{\gamma^2}{4}s^2{}_{147}\right\} r_7^{\backprime} + \frac{\gamma^2}{4}s_{147}\, r_6^{\backprime}$$

If we suppose

$$\frac{a}{r} = 1 + r_0 + e(1+f)\cos\Big(n(1+k)\,t + \varepsilon - \varpi\Big) + e_{\prime}f_{\prime}\cos\Big(n(1+k_{\prime})\,t + \varepsilon_{\prime} - \varpi_{\prime}\Big)$$

$a < a_{\prime}$ we find

$$r_0 = \frac{m_{\prime}}{\mu}\left\{\frac{a^3}{2a_{\prime}^3}b_{3,0} - \frac{a^2}{2a_{\prime}^2}b_{3,1}\right\} \qquad k = \frac{m_{\prime}}{\mu}\left\{\frac{a^3}{a_{\prime}^3}b_{3,0} - \frac{5a^2}{4a_{\prime}^2}b_{3,1}\right\}$$

$$f_{\prime}\left\{(1+k_{\prime})^2(1-3r_0) - 1\right\} = \frac{m_{\prime}a^2}{2\mu a_{\prime}^2}b_{3,2}$$

If $\quad n\{1+2r_0\}=\mathrm{n} \quad$ and $\mathrm{n}^2=\frac{\mu}{\mathrm{a}^3} \qquad a=\mathrm{a}\left\{1+\frac{4}{3}r_0\right\}$

If 2 e is the coefficient of $\sin\Big(n(1+k)\,t+\varepsilon-\varpi\Big)$ in the expression for the longitude,

$$e(1+f)=\mathrm{e}\,(1+k-r_0)$$

$$\frac{\mathrm{a}}{r}=1-\frac{1}{3}r_0+\mathrm{e}\left\{1+k-\frac{7}{3}r_0\right\}\cos\Big(n(1+k)\,t+\varepsilon-\varpi\Big)$$

$$+\,e_{,}f_{,}\cos\Big(n\,(1+k_{,})\,t+\varepsilon-\varpi_{,}\Big)$$

$$=1-\frac{m_{,}a^3}{6\,\mu\,a_{,}^3}b_{3,0}+\frac{m_{,}a^2}{6\,\mu\,a_{,}^2}b_{3,1}$$

$$+\,\mathrm{e}\left\{1-\frac{m_{,}a^3}{6\,\mu\,a_{,}^3}b_{3,0}-\frac{m_{,}a^2}{12\,\mu\,a_{,}^2}b_{3,1}\right\}\cos\Big(\mathrm{n}\Big(1-\frac{m_{,}a^2}{4\,\mu\,a_{,}^2}b_{3,1}\Big)\,t+\varepsilon-\varpi\Big)$$

$$+\,e_{,}f_{,}\cos\Big(n\,(1+k_{,})\,t+\varepsilon-\varpi_{,}\Big)$$

$$\frac{r}{\mathrm{a}}=1+\frac{1}{3}r_0-\mathrm{e}\left\{1+k-\frac{5}{3}r_0\right\}\cos\Big(n\,(1+k)\,t+\varepsilon-\varpi\Big)$$

$$-\,e_{,}f_{,}\cos\Big(n\,(1+k_{,})\,t+\varepsilon-\varpi_{,}\Big)$$

$$=1+\frac{m_{,}a^3}{6\,\mu\,a_{,}^3}b_{3,0}-\frac{m_{,}a^2}{6\,\mu\,a_{,}^2}b_{3,1}$$

$$-\,\mathrm{e}\left\{1+\frac{m_{,}a^3}{6\,\mu\,a_{,}^3}b_{3,0}-\frac{5\,m_{,}a^2}{12\,\mu\,a_{,}^2}b_{3,1}\right\}\cos\Big(\mathrm{n}\Big(1-\frac{m_{,}a^2}{4\,\mu\,a_{,}^2}b_{3,1}\Big)\,t+\varepsilon-\varpi\Big)$$

$$-\,e_{,}f_{,}\cos\Big(\mathrm{n}\,(1+k_{,})\,t+\varepsilon-\varpi_{,}\Big)$$

If $a < a_{,}$ as before, and

$$\frac{a_r}{r_{,}}=1+r_{,0}+e_{,}\,(1+f')\cos\Big(n_{,}\,(1+k')\,t+\varepsilon_{,}-\varpi_{,}\Big)+ef_{,}'\cos\Big(n_{,}\,(1+k_{,}')\,t+\varepsilon_{,}-\varpi\Big)$$

we find

$$r_{,0}=\frac{m}{\mu}\left\{\frac{1}{2}b_{3,0}-\frac{a}{2\,a_{,}}b_{3,1}\right\} \qquad k_{,}=\frac{m}{\mu_{,}}\left\{b_{3,0}-\frac{5\,a}{4\,a_{,}}b_{3,1}\right\}$$

$$f_{,}'\left\{(1+k_{,}')^2\,(1-3\,r_{,0})-1\right\}=\frac{m\,a}{2\,\mu_{,}a_{,}}b_{3,2}$$

If $\mathrm{n}_{,}\{1+2r_0\}=\mathrm{n}_{,}$ and $\mathrm{n}_{,}^2=\frac{\mu}{\mathrm{a}_{,}^3}, \qquad a_{,}=\mathrm{a}_{,}\left\{1+\frac{4}{3}r_{,0}\right\}$

$$\frac{a_{,}}{r_{,}} = 1 - \frac{m}{6\mu_{,}} b_{3,0} + \frac{m a}{6 \mu_{,} a_{,}} b_{3,1}$$

$$+ e_{,} \left\{ 1 + \frac{m}{6\mu_{,}} b_{3,0} - \frac{m a}{12 \mu_{,} a_{,}} b_{3,1} \right\} \cos \left(n_{,} \left(1 - \frac{m a}{4 \mu_{,} a_{,}} b_{3,1} \right) t + \varepsilon_{,} - \varpi_{,} \right)$$

$$+ e f_{,}' \cos \left(n_{,} (1 + k_{,}') \, t + \varepsilon_{,} - \varpi \right)$$

μ is the mass of the sun + the mass of the disturbed planet, which is not of course the same for both, but the difference may be neglected in the planetary theory.

Laplace determines the arbitrary quantity $f_{,}$, upon the hypothesis that the coefficient of the argument $\sin \left(n\,(1 + k)\,t + \varepsilon - \varpi_{,} \right)$ in the expression for the longitude equals zero. According to the received theory of the moon, the true longitude is expressed in a series of angles consisting of various combinations of the quantities t, x, y and z, and their multiples and no others; and in this theory the angle $t + z$ occupies the place of the argument $n t + \varepsilon - \varpi_{,}$, so that omitting ε which accompanies t,

$$\frac{a}{r} = 1 + r_0 + e\,(1 + f) \cos (c\,n\,t - \varpi) + e_{,} f_{,} \cos (n\,t - n_{,} t + c_{,} n_{,} t - \varpi_{,})$$

$$\frac{a_{,}}{r_{,}} = 1 + r_{1,0} + e_{,} (1 + f') \cos (c'\,n_{,}\,t - \varpi_{,}) + e f_{,}' \cos (n_{,} t - n\,t + c\,n\,t - \varpi)$$

c[*] $= 1 - \dfrac{m_{,} a^2}{4 \mu\, a_{,}^2} b_{3,1}$ $\qquad c_{,} = 1 - \dfrac{m a}{4 \mu_{,} a_{,}} b_{3,1} = 1$ nearly

$n_{,} (c_{,} - 1) = n\,k_{,} = 0$ nearly

$$f_{,} \left\{ (1 + k_{,})^2 (1 - 3 r_0) - 1 \right\} = \frac{m_{,} a^2}{2 \mu\, a_{,}^2} b_{3,2} = \frac{15\, m_{,} a^4}{8 \mu\, a_{,}^4}$$

$$r_0 = - \frac{m_{,} a^3}{2 \mu\, a_{,}^3} \qquad f_{,} = \frac{5\,a}{4\,a_{,}}$$

---

[*] c and g are determined by quadratic equations,

$$c = \frac{\sqrt{\left\{ 1 + \frac{m_{,}}{\mu} \left\{ \frac{a^3}{2 a_{,}^3} b_{3,0} - \frac{a^2}{a_{,}^2} b_{3,1} \right\} \right\}}}{1 + \frac{m_{,}}{\mu} \left\{ \frac{a^3}{2 a_{,}^3} b_{3,0} - \frac{a^2}{2 a_{,}^2} b_{3,1} \right\}} = 1 - \frac{m_{,} a^2}{4 \mu\, a_{,}^2} b_{3,1} \quad \text{nearly.}$$

This gives for the coefficient of sin $(t + z)$ in the expression for the longitude

$$+ \left\{ \frac{5\,a}{2\,a_{,}} - \frac{3\,m_{,}\,a^4}{8\,\mu\,a_{,}^4} \right\} e_{,}$$

which in sexagesimal seconds is 21″·7, according to M. Damoiseau it should be 17″·56.

Finally,

$$\frac{\mathrm{a}}{r} = 1 + \frac{m_{,}\,a^3}{6\,\mu\,a_{,}^3} + \mathrm{e} \left\{ 1 - \frac{7\,m_{,}\,a^3}{12\,\mu\,a_{,}^3} \right\} \cos x + \frac{5\,a}{4\,a_{,}}\, e_{,} \cos (t + z)$$

$$\lambda = \mathrm{n}\,t + 2\,\mathrm{e} \sin x + \left\{ \frac{5\,a}{2\,a_{,}} - \frac{3\,m_{,}\,a^4}{8\,\mu\,a_{,}^4} \right\} e_{,} \sin (t + z)$$

Substituting for $b_{3,1}$, $b_{3,2}$ their values in series

$$b_{3,1} = \frac{3\,a}{a_{,}} + \frac{3\,.\,3\,.\,5\,a^3}{2\,.\,4\,a_{,}^3} + \&\text{c.} \qquad b_{3,2} = \frac{3\,.\,5\,a^2}{4\,a_{,}^2} + \frac{3\,.\,3\,.\,5\,.\,7\,a^4}{2\,.\,4\,.\,6\,a_{,}^4} + \&\text{c.}$$

$$\mathrm{c} = 1 - \frac{3\,m_{,}\,a^3}{4\,\mu\,a_{,}^3} \qquad \mathrm{c}_{,} = 1 - \frac{3\,m\,a^2}{4\,\mu_{,}\,a_{,}^2}$$

I have shown, Phil. Trans. 1832, p. 38, that when $a < a_{,}$

$$g = 1 + \frac{m_{,}}{\mu} \left\{ \frac{a^3}{a_{,}^3}\, b_{3,0} - \frac{3\,a^2}{4\,a_{,}^3}\, b_{3,1} \right\}$$

$$g\,n = \mathrm{n} \left\{ 1 + \frac{m_{,}\,a^2}{4\,\mu\,a_{,}^2}\, b_{3,1} \right\}$$

Similarly it may be shown that

$$g_{,} = 1 + \frac{m}{\mu_{,}} \left\{ b_{3,0} - \frac{3\,a}{4\,a_{,}}\, b_{3,1} \right\}$$

$$g_{,}\,n_{,} = \mathrm{n}_{,} \left\{ 1 + \frac{m\,a}{4\,\mu_{,}\,a_{,}}\, b_{3,1} \right\}$$

The arguments

$$n\,t - \nu,\ n\,t - \nu_{,},\ n\,t_{,} - \nu_{,} \text{ and } n_{,}\,t - \nu$$

occupy the same place in the expression for the latitude as

$$n\,t - \varpi,\ n\,t - \varpi_{,},\ n_{,}\,t - \varpi_{,} \text{ and } n_{,}\,t - \varpi$$

in the expression for the radius vector. Similar methods may be employed to determine the arbitrary quantities, so that no other angles occur in the expression for s except the quantities t, x, z, y, and if the quantities c and g are rational, no imaginary angles can be introduced.

XVIII. *On the Nervous System of the* Sphinx ligustri, Linn., *and on the changes which it undergoes during a part of the Metamorphoses of the Insect.*

By George Newport, *Esq. Communicated by* P. M. Roget, *M.D. Sec. R.S.*

Read June 7, 1832.

In this paper it is proposed to describe the development and arrangement of the nerves, and the changes which they undergo, in the *Sphinx ligustri*, Linn., during the last stage of the larva, and the earlier stages of the pupa state.

The labours of that industrious naturalist Heroldt have already shown us, to a certain extent, in what manner similar changes occur in the *Papilio brassicæ*, Linn.; and therefore the author of the present essay would not have ventured to trespass upon the attention of the Royal Society, were it not that these changes are capable of more minute explanation than those which take place with such rapidity in the *P. brassicæ*. But the *Sphinx ligustri*, Linn., remaining as it does for several months in an apparently torpid condition, between its larva and perfect state, allows us an opportunity of more deliberately observing in what manner the changes are effected; while the superior bulk of the insect enables us to trace them with greater precision.

The *Sphinx ligustri*, like other Lepidopterous insects, after coming from the egg, has three very distinct periods of existence, recognised as the larva, the pupa, and the perfect state. In the larva state there are also distinct periods, terminated by the change of skin which takes place at the expiration of each. This change of skin occurs six times before the insect passes into the pupa state. After each change the larva becomes much enlarged, feeds more voraciously than at any preceding period, and when arrived at the sixth and last, which is always of longer duration than the earlier ones, increases so rapidly in bulk as to become at least a third larger than at any earlier period. Its nervous system undergoes a corresponding development. In every stage it is composed of two longitudinal cords, united at certain distances by ganglia. Of these there are now eleven, [Plate XII. fig. 1. (1, 2 to 11),] besides a nodulated mass in the head which is supposed to represent the brain, [fig. 1. (A), fig. 2. (A).] This mass lies above the œsophagus, and is formed of two lobules closely united, convex upon their upper, and a little concave on their under surface, so as in the middle line to accommodate themselves to the anterior part of the dorsal vessel, which passes immediately beneath them, and to the œsophagus along which this is directed. The longitudinal cords originate from the under surface of these lobules, [Plate XII. fig. 2. (G),] and passing a little backwards meet beneath the œsophagus, and, by their uniting, form the heart-shaped or first ganglion, [fig. 1. (1), fig. 2. (H, 1).] From this they are continued close to each other into the next segment, or true collar of the future moth, and here connected form the second ganglion, [fig. 1. (2),] which is nearly of a spherical form. The cords then gradually diverge, and proceed apart from each other, passing on the outside of, and inclosing between them the insertions of some of the diagonal muscles of the future thorax, until they again unite in a third and distinctly bilobate, heart-shaped ganglion, [fig. 1. (3).] From this they are continued in the same manner into the fourth segment, and uniting form a similarly-shaped fourth ganglion, [fig. 1. (4).] They then pass close to each other into the anterior part of the fifth segment, and form a ganglion, [fig. 1. (5),] the distance of which from the fourth, like that of the second from the first, is scarcely more than half of what exists between any of the other ganglia. From the fifth they are continued to the sixth, seventh, and so on to the eleventh segments, forming in the middle of each, one nearly spherical ganglion, [fig. 1. (5, 6, 7, 8, 9, 10, 11),] which has scarcely any appearance of having originally been formed of two lobes. The eleventh ganglion, however, is distinctly bilobate, [Plate XII. fig. 1. (11),] and at this period of the larva's existence is in reality a double ganglion, with a constriction in its middle, which is more or less apparent in different individuals; so that, as was suggested to me by Dr. R. E. Grant, it is highly probable this eleventh, or terminal ganglion, consisted originally of two separate ganglia, with short intervening cords. This is the more probable as there are no ganglia, or cords, in the twelfth and anal segments, the parts being supplied with nerves directly from the terminal ganglion. This opinion is also supported by the fact, that in the larva of several other moths, particularly that of the *Phalæna* (*Bombyx*) *neustria*, Linn.,

Fig. 2.

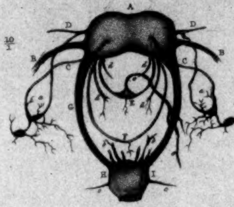


Fig. 1.

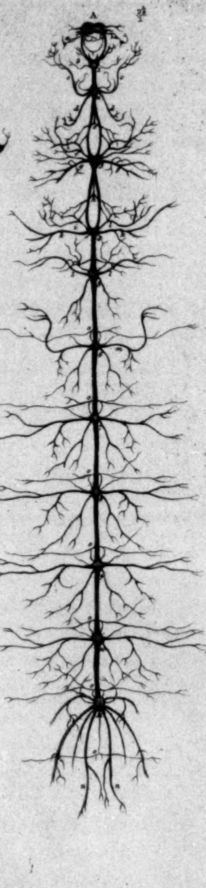


Fig. 4.

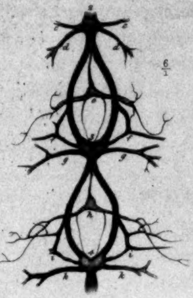


Fig. 3.

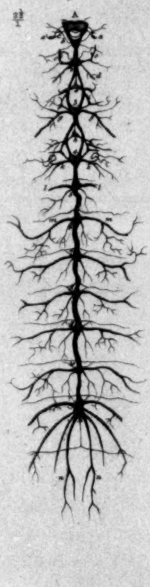
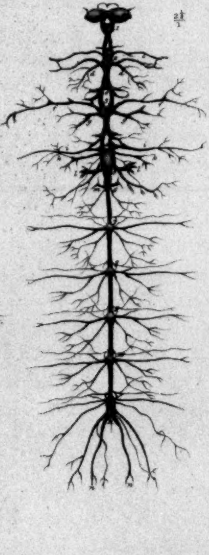


Fig. 5.



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J. Basire sc.

Fig. 1.

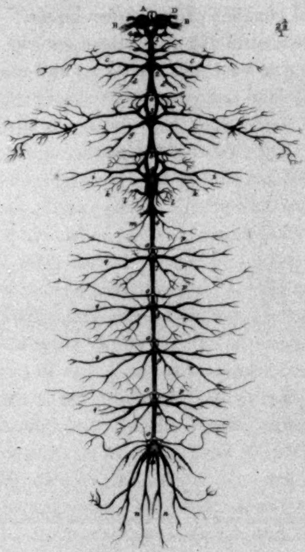
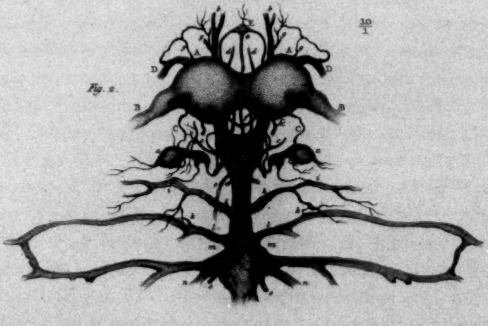


Fig. 2.



G. Newport del.

J. Basire sc.

there are two very distinct ganglia, with intervening cords, which afterwards unite to form the terminal ganglion of the perfect insect.

In describing the nerves distributed from these ganglia, it may be well to consider them as belonging to the head, the thorax and abdomen. In the first there are the cerebral lobes and first ganglion, which are found in the head in every period of the insect's existence, but undergo a modification of form, are increased in diameter, and furnish nerves to the organs of sense and manducation. The second division comprehends the ganglia which furnish nerves to the true limbs, or organs of motion. These ganglia are contained in the second, third, fourth and fifth segments of the larva, which correspond to the collar and trunk of the pupa and perfect insect. The third or abdominal division comprises the ganglia in the eight last segments in the larva, and the corresponding ones in the pupa and perfect state. The cords in this division are much shortened, and the number of the ganglia diminished, during the change of the insect from the larva to the perfect state.

Nerves of the Head.—When viewed from above, the cerebral lobes are pretty uniform in appearance, and are clearly distinguished from each other by a depression between them. This is more apparent on the anterior than the posterior surface, and arises from the lateral part of each lobe being carried a little forwards, so that the two lie across the œsophagus in a curved or semilunar direction. From the anterior and lower part of each lobe originate four remarkable nerves. Two of these [Plate XII. fig. 2. (*d, d, d, d*)] are distributed towards the front of the head, near the flexor muscles of the mandibles; a third passes a little forwards, descends, and, uniting with its fellow from the opposite lobe, forms a circle [fig. 2. (F)] round the œsophagus, to the under surface of which it distributes a few filaments; while the fourth, which originates rather higher up than the others, forms what has been called by LYONNET the recurrent ganglion and nerve, [Pl. XII. fig. 2. (E); Pl. XIII. fig. 2. (E).] From its origin, this nerve is directed forwards and downwards, along the side of the œsophagus, or rather posterior part of the mouth, but gradually altering its course inclines upwards and inwards, and then a little backwards, until, by meeting its fellow of the opposite side above the roof of the mouth, the two by their union form a semilunar ganglion [Pl. XII. and XIII. fig. 2. (E)] immediately below the bifurcated portion and distribution of the dorsal vessel.

From the front, or most convex surface of this ganglion, originates a small branch that distributes filaments in the direction of the superior lip; while a large nerve is produced from the posterior surface, [fig. 2. (*e*, *e*),] which passes backwards beneath the cerebral lobes, along the middle of the œsophagus, covered by the dorsal vessel. On arriving at the stomach, it divides into three branches, [Pl. XII. fig. 2. (E, *e*); Pl. XIII. fig. 2,] which are distributed chiefly to that organ. Throughout the whole of its course, from the ganglion to this division into branches, it distributes filaments to the dorsal vessel and to the œsophagus. I have not yet succeeded in tracing it in this insect beyond the anterior part of the stomach, but in the *Gryllus viridissimus*, LINN., I was once enabled to follow its central division along the whole of the stomach, and part of the small intestine, from which, with a little care, it was readily detached. Its length from the ganglion to the trifid division in the Sphinx, is much increased during the changes of the insect, and corresponds precisely with the elongation that takes place in the œsophagus. The form of the ganglion undergoes no alteration. From the analogy that exists in the distribution of this nerve to that of the eighth pair in the vertebrated animals, it is probable that its functions are of a somewhat similar nature,—that in reality it is the par vagum, or pneumogastric nerve of insects. In fact, this is the pretty generally received opinion respecting it, and is clearly that of STRAUS DURCKHEIM, who describes it in his Anatomy of the *Melolontha vulgaris*. It must be confessed, however, that there are objections to such an opinion, since it is not yet proved to distribute any filaments to the respiratory organs, although it can hardly be doubted that such distribution does really exist, when we remember the abundance of tracheal vessels which ramify upon the stomach, and with which its filaments must necessarily come in contact. The other nerves from the cerebral lobes arise nearer the lateral surfaces. The first of these are destined for the future antennæ, and proceed from the front, near the origin of the cords, [Pl. XII. and XIII. fig. 2. (D).] At the last period of the larva state they are of considerable size and length, and lie packed in sigmoid folds on each side the head, within the cranium. The next are the optic nerves. These come from the upper part of each lobe, [Pl. XII. and XIII. fig. 2. (B),] and in the larva are scarcely more than slender cords directed diagonally outwards to the six minute eyes. In addition to these nerves from the cerebral lobes there are also two minute pairs which form very remarkable ganglia, similar to those described by **Straus Durckheim** in his Anatomy of the Melolontha. These ganglia I have ventured to call anterior lateral ganglia. The two pairs of nerves originate, one from the base of the nerve to the antennæ, the other from the posterior surface of each lobe, [Pl. XII. and XIII. fig. 2. (*a*, c).] They are directed backwards and outwards, and after passing for some distance unite and form an irregular lunated ganglion, which is closely connected to another of an oval form. Both these ganglia distribute filaments to the muscles of the neck and to a lateral branch of the dorsal vessel, and are connected with a system of nerves derived from the large ganglion in the second segment, [Pl. XIII. fig. 2. (*a*, c, *i*).]

All the nerves which supply the organs of motion belonging to the head and mouth, excepting only those to the antennæ, derive their origin from the first ganglion. There are four distinct pairs; three of which proceed from the anterior, and one from the lateral surface of the ganglion. The largest pair from the anterior surface are divided into three branches, and go directly to the mandibles [Plate XIII. fig. 2, (*b*, *b*)]; the next to the palpiform spinnerets [fig. 2. (*d*, *d*)]; and the third apparently to the inferior lip; while the lateral pair [fig. 2. (*c*, *c*)] are given exclusively to the silk-bags, which afterwards are the salivary vessels of the perfect insect.

Nerves of the Thorax.—These arise from the second, third, fourth and fifth ganglia, and their intervening cords, [Plate XII. and XIII. fig. 1. (2, 3, 4, 5).] The first pair from the second ganglion are remarkably small in the larva, and their distribution is not easily traced. The second are large, directed forwards, and divided into many branches, which supply the muscles of the head and neck, [fig. 1. (*c*, *c*).] The third are carried backwards for a little distance, and then turning forwards enter the first pair of legs, [fig. 1. (*d*, *d*).] Both the cords between the second and third ganglia produce a single nerve, which is directed backwards, and unites at an angle with the first nerve from the third ganglion, [fig. 1. (*f*, *f*).] These form a single trunk, which goes to the first pair of wings in the perfect insect. It is now of small diameter, but is carried forwards and distributes filaments among the muscles at the anterior part of the segment. This trunk is also connected with a system of nerves of which we shall speak more particularly hereafter. The second pair from the third ganglion, [Plate XII. fig. 1. (3, *g*, *g*),] distribute from their base a small branch, which looks like a distinct nerve, while their main trunks, at a distance from the ganglion, divide into two branches, and are given to the second pair of legs. The cords between the third and fourth ganglia produce also a nerve that unites, in a manner similar to the preceding, with the first nerve from the fourth ganglion, [fig. 1. (4, *i*, *i*),] and forms a trunk which ramifies among the muscles of the fourth segment, and is destined for the second pair of wings. The second pair of nerves from the fourth ganglion [fig. 1. (4, *k*, *k*),] are given to the third pair of legs. The nerves from the fifth ganglion [fig. 1. (5, *l*, *l*)] belong also to the thorax, and are those which are given to the muscles of the hinder part of the thorax in the perfect insect.

Nerves of the Abdomen.—All the nerves from the sixth to the terminal or eleventh ganglion, belong to this division, and, with the exception of those from the latter, are pretty nearly uniform both in number and distribution. Each ganglion produces one pair of small nerves, and one of large. The small ones are given to the fat and minute tracheæ of the ventral surface. The large ones pass transversely across the segments, and divide each into two branches. One of these [Plate XIII. fig. 1. (*q*, *q*, *q*, *q*)] passes over the inner range of fibres and between the layers of abdominal muscles, and following the course of the trachea gives its branches to the dorsal muscles, and to the integuments of the back; while the second, [fig. 1. (*r*, *r*, *r*, *r*),] passing also between the layers of ventral muscles, distributes its branches to their inner surface, and to the integuments of the under surface of the body. The eleventh or terminal ganglion [Plate XII. fig. 1. (11)] produces five pairs of nerves, four of which are of considerable size. These are arched backwards, and three of them are given to the remaining segments of the body, while the others supply the colon, rectum, and rudiments of the organs of generation.

Besides the nerves thus described, as constituting those of the head, thorax and abdomen, there are others which merit some attention, as they seem to form a distinct or superadded series. LYONNET has accurately delineated them in his excellent Anatomy of the larva of the Cossus. There is a plexus of them lying transversely in every segment, attached by apparently a single filament, passing between the longitudinal cords to the posterior part of every ganglion, [Plate XII. and XIII. fig. 1. (*e*, *h*, *o*, *o*, *o*, *o*, *o*).] Some of the nerves from each plexus in the abdomen unite with the principal nerve from the next ganglion [Plate XIII. fig. 1. (*q*, *q*, *q*, *q*)], while others ascend and ramify among the tracheæ and dorsal muscles. The principal branch [fig. 1. (*p*, *p*, *p*, *p*)] goes directly to the tracheæ which come from the spiracula. In the thorax, the plexus from the hinder part of the second ganglion, [Plate XII. and XIII. fig. 1. (2, *e*),] unites some of its filaments with the nerve destined for the first pair of wings, while others are distributed among the muscles. The nerves from the plexus attached to the third ganglion give, in a similar manner, some of their filaments to the nerve intended for the second pair of wings, and some to the muscles. The second ganglion has the transverse plexus from the first, attached pretty closely to its anterior surface. This plexus distributes its nerves laterally to the muscles of the head and neck. It is also united by a small branch with the anterior lateral ganglia, [Plate XIII. fig. 2. (*a*, *i*),] and with the first pair of nerves from the second ganglion, [fig. 2. (*k*, *k*, *l*, *l*),] so as to form a complete link or medium of communication between the nerves and ganglia of the head, neck, and second segment. From this it seems probable that these nerves may constitute the origin of a distinct system; but whether this system in insects be analogous, either to the sympathetic or to the respiratory system of vertebrated animals, is yet a matter of inquiry. From the principal branches from each abdominal plexus being always distributed among the tracheæ, near the spiracula, there seems reason for inclining to the latter opinion.

Such is the arrangement of the nervous system when the larva has attained its full growth, and ceased to eat, preparatory to its changing into a pupa. This generally takes place at about the middle of August, or in the beginning of September. Some individuals undergo the change three weeks or a month earlier than others, owing to their having been developed from the egg at an earlier period; and the length of time they continue in the state of larva is about seven or eight weeks. The first symptoms of the insect being about to change to the pupa state occurs in its ceasing to eat, and after having remained quiet for a few hours, becoming exceedingly restless, and moving about with great rapidity. It then enters the earth, and forms an oval cell lined with a thin silky coating, and in it awaits its change. The delicate pea-green skin of the larva now becomes of a rusty orange colour, is shrivelled and contracted, and is often covered with moisture. At this period all the nerves belonging to the ganglia of the first five segments are directed forwards, [Plate XII. fig. 1.,] while the lateral nerves from the ganglia in the posterior segments are disposed with some irregularity. If the larva be prevented from undergoing its metamorphosis, by having been removed from its cell in the earth, and also prevented from remaining at rest, the nervous system is but little altered. But the change can be retarded only for two or three days, when the insect, upon being allowed to remain at rest for a short period, entirely loses the power of locomotion, becomes much shortened and contracted, and in its general appearance resembles the future pupa. When the period of change has arrived, the larva forces an opening through its skin, along the dorsal part of the third and fourth segments, and by repeated efforts continues it forwards as far as the head, the covering of which separates into three pieces. The head and trunk of the new pupa, with all their parts separate, but encased each in a very delicate skin, and nearly as fluid as water, are then gradually withdrawn from beneath the old covering, and disposed along the under surface of the body, while the skin itself, by repeated contractions of the abdomen, is forced up together, and entirely slipped off at the anal segment. The new pupa then lies at rest, and the coverings of its limbs and body adhere together and form a hard case, capable of preserving it from injury. During this, its nervous system is also changing, by the cerebral lobes being increased in size, and the eleven ganglia brought nearer together, by the shortening which is taking place in their respective segments, so that the longitudinal cords lie in a very irregular manner between the ganglia, while the ganglia themselves are confined in their proper places in the segments by the nerves running transversely from them.

If the insect be examined about two hours previously to its bursting the exuviæ and becoming a pupa, the change in its nervous system is very evident, [Plate XII. fig. 3.] The lobes above the œsophagus have assumed more the appearance of a cerebral mass, [fig. 3. (A),] and are increased in diameter, while the cords descending from them are shortened and thickened. The nerves for the antennæ are enlarged, and lie folded on each side the head, and the optic nerves have undergone considerable alteration. Instead of being simple cords, they are now so much shortened and thickened, as to be hardly distinguishable from the lobes themselves, upon the upper part of which they are situated, while an ovate patch of a purplish substance is observed upon their surface. This has existed in every specimen I have dissected, and seems to form part of what is to become the dark pigment for the eye of the future moth. The ganglion that produces the nerve to the œsophagus and stomach has undergone no alteration, nor have the anterior lateral ganglia, [Plate XII. fig. 3. (*a*, *a*),] and there is still a loop or nervous ring around the anterior part of the œsophagus, as in the perfect larva. There are still eleven ganglia [fig. 3. (1 to 11)] upon the longitudinal cords; but none of these are yet increased in size, nor is there any particular alteration in the distribution of the nerves from the six abdominal ones, while the cords are still disposed in an irregular manner, and have not increased in diameter. But in the thorax the nerves are much enlarged, particularly those sent to the wings, while in some instances the nerves belonging to the posterior pair of legs are curiously convoluted within the thorax, preparatory to their being unrolled at the instant of the change to the pupa, [fig. 3. (4, *k*).] The superadded or transverse plexus of nerves are also enlarged, particularly at the points of union with the filaments which connect the plexus with the ganglia. They are occasionally so much increased at those points as to form distinct but irregularly shaped ganglia, nearly one third the size of the longitudinal ones of the cord, [Plate XII. fig. 4. (*e*, *h*).] The lateral branches of the plexus have sometimes minute ganglia, [fig. 4. (*e*),] from which the nerves are produced; but this is not often the case.

Four days after the insect has become a pupa, the nervous system is much in the same state as at the moment of transformation, the chief difference being in the fifth ganglion having advanced nearer to the fourth, and become more indistinct, while the diameter of the cords has increased, so as to equal the whole diameter of the ganglion. The cerebral lobes, optic nerves, and anterior lateral ganglia, as well as the ganglia of the trunk and abdomen, continue nearly in the same state as before, and the cords, although a little shortened, are still irregularly disposed in the abdomen.

Thirty days after the change there is a considerable alteration in the nervous system, [Pl. XII. fig. 5.] The cerebral lobes are more increased, the optic nerves a little extended, and the first ganglion has been brought so very close to the lobes as to appear to constitute with them a single mass, through which there is a small aperture for the passage of the œsophagus. All the ganglia of the thorax are much enlarged, and the first pair of nerves which belonged to the second ganglion in the larva, now appear to take their origin from the cords, [fig. 5. (2, *c*),] and after anastomosing with the second pair, to form with them a plexus which supplies the neck and collar; while the third pair pass as before to the muscles of the first pair of legs, [Pl. XII. fig. 5. (2, *d*).] The first pair from the third ganglion, and the roots they derive from the cords, are much enlarged, [fig. 5. (3, *f*),] as also are the second pair given to the second pair of legs. But the greatest alteration is in the fourth ganglion, [fig. 5. (4),] which is now more than double its former size, is elongated and bilobate, and gives off four pairs of nerves. The first, with the roots they derive also from the cord, are given to the inferior wings; the second, to the third pair of legs; the third pass backwards to the muscles of the abdomen; and the fourth are directed upwards, divided into three branches, and are distributed to the posterior muscles of the trunk. The fifth ganglion is close to the fourth, [fig. 5. (5),] and coalesces with it; the nerves last described being those which originally belonged to it. The sixth ganglion, [fig. 5. (6),] much decreased in size, is often found at this period close to the fifth, from which it is separated only by a slight indentation. It is more frequently, however, at a short distance from it. The longitudinal cords are no longer irregularly folded in the abdomen; they now lie in a direct line between the ganglia, [fig. 5. (7, 8, 9, 10, 11)]; but neither these nor the cord itself are increased in diameter.

It is thus evident that the principal part of the change in the nervous system of this insect occurs during the first month of the pupa state, and that it is not regularly progressive, but takes place at intervals. Upon what these apparent irregularities depend it is difficult to determine. Perhaps they may be the result of a partial exhaustion of the vital powers, during the effort of transformation, and which require an interval of repose to re-establish their activity. Thus we find, that during the first four days of the pupa state, there is but little alteration of structure, beyond what exists at the actual period of changing from the larva; the energy of the insect having been partially exhausted during the effort of transformation. But when it has remained for some time at rest, its energy is restored, and the change again advances. That such is in reality the case seems to be supported by the fact, that when a larva has become so exhausted as to be unable to rid itself of the exuviæ, and complete its transformation, owing to its having been prevented from remaining at rest during the proper period, the change in its nervous system is never so much advanced as in those which have transformed without interruption ; nor does it make any further progress even in seven days, while the insect itself generally perishes in less than a fortnight.

After the insect has remained for about five weeks in the pupa state, scarcely any further change occurs in its nervous system until the following spring. This period of repose, during which the insect remains nearly torpid in its cell in the earth, continues for at least twenty-three or twenty-four weeks, and extends in general from the middle of October to the beginning of the following March, when, if the season be temperate, the change again advances. If the pupa be examined at any time during this interval, scarcely any alteration is observed in its nervous system, except an enlargement of the anterior lateral ganglia.

In the beginning or middle of March, when the pupa is becoming more lively, a change in the nervous system is evidently in progress. The cerebral lobes, [Pl. XIII. fig. 1. & 2. (A, A),] when viewed from above, are distinct from each other, are increased in size, and are of an irregular spherical figure. The ganglion called the recurrent, [fig. 2. (E),] rests immediately above a semi-cartilaginous arch, that forms the upper part of the mouth, while its nerve passes backward as before, distributing its filaments to the œsophagus, and anterior part of the dorsal vessel. The nerves to the antennæ are still in the same state as before, but a small branch [fig. 2. (D, *f*)] may now be observed coming from their base, and directed downwards towards the mouth, and apparently connecting itself with the filaments from the nerves which belonged to the mandibles, and also to one from the anterior lateral ganglia, [fig. 2. (*g*).] The optic nerves are extending, and are greatly enlarged at their base, [fig. (B, B),] but there is no enlargement of the patch of dark pigment, [fig. 2. (B, *c*).] The nervous circle still exists around the anterior of the œsophagus, [fig. 2. (F),] and the anterior lateral ganglia are greatly increased in size, and still originate in the same manner as in the larva. But the nerves they distribute, and the connexions they form with other nerves, are more easily detected at this than at an earlier period. The first one, the lunated ganglion, [fig. 2. (*c*),] distributes several minute nerves, one of which from its inner angle passes backwards, and is connected with the plexus of transverse nerves from the first ganglion of the trunk, from which plexus there are also filaments that unite with the first pair of nerves from the same ganglion, and thus establish a direct communication with the cerebral lobes. The other ganglion, the oval one, [fig. 2. (*a*),] is larger than the lunated, and distributes several branches. The distribution of nerves from the ganglia of the trunk and abdomen remains nearly the same. The transverse plexus have a little increased in size, and their union with the transverse nerves of the cord takes place a little nearer the ganglia.

I have thus described the structure and arrangement of the nervous system in the larva of the Sphinx, and the development which it undergoes during the earlier stages of the pupa state. In a subsequent paper, which I hope to have the honour of laying before the Society, these changes will be followed through the remaining stages, and some observations submitted upon the manner in which they are effected.

May 22, 1832.

Description of the Plates.

PLATE XII.

Fig. 1.—Nervous system of the larva of *Sphinx ligustri*, after it has acquired its full growth, and about two days previously to its change to the pupa state. Magnified two diameters and a half.

A. The supposed brain or anterior nodules of the cord.

1. The first ganglion situated in the head, or first segment beneath the nodules.

2, 3, 4, 5. Ganglia of the trunk supplying nerves to the legs and wings.

6, 7, 8, 9, 10, 11. Ganglia of the abdomen.

a. The anterior lateral ganglia. *b.* Nerves to the mandibles. *c.* Second pair from the second ganglion given to the muscles of the neck. *d.* Third pair given to the first pair of legs. *e.* The plexus of transverse or superadded nerves from the second ganglion. *f.* Nerves to the first pair of wings, originating from two roots, one from the cord and one from the third ganglion, and connected also with the transverse plexus. *g.* Second pair of nerves from the third ganglion given to the second pair of legs. *h.* Transverse plexus from the third ganglion. *i.* Nerves to the second pair of wings originating like the first, from two roots, one from the cord and one from the fourth ganglion, and connected also with branches from the transverse plexus from the third. *k.* Second pair from the fourth ganglion given to the third pair of legs. *l.* Nerves from the fifth ganglion, which, in the pupa, are those given to the posterior muscles of the trunk. *m.* Nerves from the sixth ganglion, which, in the pupa, are those of the anterior muscles of the abdomen. *n.* The last pair of nerves from the terminal ganglion given to the rectum and organs of generation.

Fig. 2.—The anterior lobes or brain, with the first ganglion and nerves of the head. Magnified ten diameters.

A. The lobes. B. The optic nerves. C. The nerves of the anterior lateral ganglia, one attached to the posterior surface of the lobes, the other to the base of those to the antennæ. D. Nerves for the antennæ. E. The singular nerve which has hitherto been called the recurrent, but which appears analogous to the par vagum or pneumogastric nerve. F. The nervous circle formed by the union of two nerves that originate near the preceding, and surround the pharyngeal or anterior portion of the œsophagus, to the under parts of which they distribute a few filaments. G. The crura or cords which descend on each side the œsophagus and connect the superior lobes with the first ganglion. H. The first ganglion. *a.* The anterior lateral ganglia. *b.* Mandibular nerves. *c.* The lateral nerves of the first ganglion given to the salivary vessels or silk-bags.

Fig. 3.—Nervous system of *Sphinx ligustri* as found at about two hours previous to its change to the pupa state. Magnified two diameters and a half.

2, 3, 4, 5. Nerves of the trunk. 6 to 11. Nerves of the abdomen. The letters indicate the same parts as those in the larva in the preceding figures.

Fig. 4.—Nerves and ganglia of the trunk, exhibiting more clearly the form of the latter, and the gangliform appearance of the transverse plexus. Magnified six diameters.

2, 3, 4. The second, third, and fourth ganglia. The letters correspond with those of the preceding figures.

Fig. 5.—Nervous system of *Sphinx ligustri* thirty days after changing to the pupa state. Magnified two diameters and a half.

This drawing exhibits the abdominal cords in their shortened state, with only five instead of seven ganglia, the fifth and sixth having passed onwards and become continuous with the fourth. The cords in the trunk and the nerves to the wings are enlarged; and those nerves which were, in the larva, the first pair of the second ganglion, are also enlarged, and now originate from the cords, while the first ganglion has advanced very near to the superior lobes or brain. The terminal ganglion exhibits a very peculiar structure. The letters refer as before.

PLATE XIII.

Fig. 1.—The nervous system of *Sphinx ligustri* as found in the pupa about the middle of March, when beginning to revive from its previous torpidity. Magnified two diameters and a half.

A. The cerebral nodules. B. The optic nerves. D. Nerves to the antennæ. 1. The first ganglion. 2, 3, 4, 5. Ganglia and nerves of the trunk. 6 to 11. Abdominal ganglia.

a. Anterior lateral ganglia. *c*. The remarkable anastomosis of the nerves which were originally the first and second pairs of the second ganglion in the larva, the first of which now arise from the cords. They supply the muscles of the collar, or true thorax of the pupa. *d*. The last pair from the second ganglion given to the first pair of legs. *e*. Posterior plexus of the transverse nerves from the second ganglion. *f*. Nerves to the first pair of wings, originating as before from the cord and ganglion, and given to the muscles of the wings in the anterior part of the trunk. *g*. Nerves of the second pair of legs. *h*. Plexus from the third ganglion. *i*. Nerves to the second pair of wings, originating and anastomosing as before. *k*. Second pair of nerves from the large or fourth ganglion, given to the third pair of legs. *l*. Nerves which originally belonged to the fifth ganglion in the larva, and which are now the posterior nerves of the trunk. *m*. Nerves from the sixth ganglion given to the anterior muscles of the abdomen. *n*. Last pair of nerves from the terminal ganglion. *o, o, o, o, o*. Transverse plexus of the abdominal ganglia, much larger than in the larva. *p, p, p, p, p*. Those branches which ramify among the dorsal muscles and tracheæ, but chiefly the latter. *q, q, q, q, q*. The superior branches of the transverse nerves from the abdominal ganglia, which, after uniting with a branch from the transverse plexus, pass upwards to the dorsal muscles. *r, r, r, r, r*. The inferior branches from the same, given to the layer of ventral muscles and inferior tracheæ.

Fig. 2.—The ganglia and nerves of the head and collar, or true thorax of the pupa, as existing in the month of March. Magnified ten diameters.

In this figure the parts are represented in situ as seen when viewed from above.

A, A. The cerebral lobes. B, B. The optic nerves, considerably enlarged at their base, where there is deposited upon the upper surface, and partly upon the lobes, the patch of dark pigment. C, C. The nerve, which attached at the base of the antennal nerve connects it with the largest of the anterior lateral ganglia, while the other, the lunated one, is attached by a filament to the posterior surface of the cerebral lobes, near where they are united. D. Nerves to the antennæ. E. The ganglion formed above the mouth by two nerves from the lower part of the front of the lobes, and which gives its nerve *e* along the œsophagus to the stomach, and which has been called the recurrent nerve.

a. Anterior lateral ganglia. *b*. The mandibular nerves which are now given to the tongue. *c*. Lateral nerves from the first ganglion, given to the salivary organs or silk-bags of the larva. *d*, *d*. Nerves from the front of the first ganglion, given to the inferior surface of the cavity of the mouth, into the substance of which they enter. They seem to be those which, in the larva, supplied the spinneret or excretory of the silk-bags. *e*. The nerve to the stomach. *f*. The nervous circle arising from the part of the lobes near the recurrent nerves, and, as in the previous state of the insect, encircling the pharyngeal portion of the œsophagus, at the hinder part of the mouth. *g*, *g*. Small nerves from the base of the antennæ passing downwards and uniting with a branch from the trigeminal or true mandibular nerves, and also with the nerve from the anterior lateral ganglia. *h*. Anterior plexus of transverse nerves from the second ganglion. *i*. A nerve connecting the plexus with the anterior lateral ganglia. *k*, *k*. Nerves connecting the plexus with the first pair from the second ganglion. *l*, *m*. Nerves which anastomose and supply the muscles of the collar. *n*. Nerves to the first pair of legs. *o*. Branches of tracheal vessels ramifying over the surface and entering into the substance of the nerves and ganglia.

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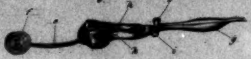

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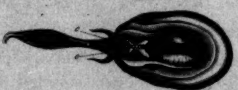

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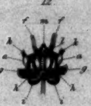

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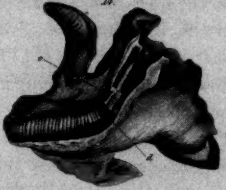

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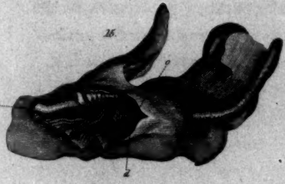

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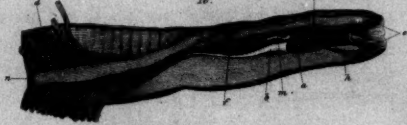

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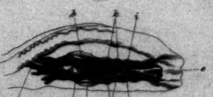

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XIX. *On the Correction of a Pendulum for the Reduction to a Vacuum: together with Remarks on some anomalies observed in Pendulum experiments.* *By* F. Baily, *Esq. F.R.S. &c. &c. &c.*

Read May 31, 1832.

The great importance which has, of late years, been attached to experiments on the pendulum, is evinced not only by the repeated and valuable labours of several of the most distinguished mathematicians and experimentalists of the present age, but also by the numerous scientific voyages that have been undertaken by several of the European Governments, with a view to ascertain and compare the results of different pendulum experiments made in various parts of the globe; and thence to determine the true figure of the earth. These results, or the number of vibrations which are made in a mean solar day, whether made by the same, or by different pendulums, were considered, till within these few years, as strictly comparable with each other by means of certain well known corrections; whereby they were reduced 1° to arcs indefinitely small, 2° to a common standard of temperature, 3° to a vacuum, and lastly to the level of the mean height of the sea.

M. Bessel, however, has recently proved that the formula for the *reduction to a vacuum* is very defective: and Dr. Young has shown that the formula for the reduction to the level of the sea is, in many cases, too great: whilst Captain Sabine has, in a paper recently published in the Transactions of this Society*, shown that there is reason to suspect the accuracy of the usual formula for the reduction to indefinitely small arcs. This latter gentleman had previously, in another work†, pointed out the discordant results arising from the use of different agate planes with the same knife edge: and had also stated his decided opinion on the powerful effect of certain geological strata in the immediate neighbourhood of the pendulum; and has even imagined that the results may be affected by an increase of buildings in the vicinity. But, to whatever cause the observed anomalies may be owing, I must confess that I have myself, during a long course of experiments on various pendulums, at different seasons of the year, and under a variety of circumstances, frequently met with discordancies that have baffled every attempt at explanation by any of the known laws applicable to the subject: and I believe that other persons also, who have had much practice in pendulum experiments, have occasionally met with anomalies for which they have been unable to account satisfactorily. As it is desirable, however, that these difficulties should be cleared up if possible, and as every information connected with so important a subject, founded on such delicate experiments, must add to our means of removing them, I trust I need not apologize for drawing the attention of this Society to the results of some experiments, made with pendulums of various forms and construction, immediately bearing on the discordancies in question.

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* Phil. Trans. for 1831, pages 467—469.

† An Account of Experiments to determine the figure of the earth; 4to, London 1825; pages 190 and 371.

In fact, till we can construct two pendulums, that will always tell precisely the same tale, cleared of all these discordancies, the important problem of the determination of the length of the seconds pendulum cannot be considered as fully solved: neither can the observations of different experimentalists, in different parts of the globe, with different pendulums, be strictly and directly comparable with each other. It is true that we have two pendulums, in form and construction totally different from each other, whose results have been closely compared: viz. **Borda**'s pendulum, and **Kater**'s convertible pendulum. But, although the great accordance in those results, by two such different means, evince the talent and skill of the distinguished persons engaged in making the experiments; yet it should now be borne in mind that the reductions to a vacuum were, in both cases, made agreeably to the old formula: and that, since M. **Bessel**'s important investigations on this subject, which indicate the necessity of revising the computations of all preceding experiments, no rigid comparison of the results has yet been repeated. The amount of the additional correction, for the two respective cases, varies materially, as I shall more fully show in the sequel: so that we are, in fact, at the present moment, totally ignorant whether the results of any two pendulums that have ever yet been constructed, are in strict and reasonable accordance with each other. And until this is practically accomplished, and can be practically repeated, I do not think that the true length of the seconds pendulum can be considered as satisfactorily determined.

Reduction to a vacuum.

M. **Bessel** has shown, in his very interesting work on the pendulum *, that the usual formula for the reduction to a vacuum, as far as the specific gravity of the *moving body* is concerned, is very defective; and by no means expresses the whole of the correction which ought to be applied: in fact, that a quantity of *air* is also set in motion by, and adheres to, the pendulum (varying according to its form and construction), and thus a *compound pendulum* is in all cases produced, the specific gravity of which will be much less than that of the metal itself. He states (page 32) that "if we denote by m the mass of a "body moving through a fluid, and by m' the mass of the fluid displaced "thereby, the accelerating force acting on the body has, since the time of "**Newton**, been considered equal to $\frac{m-m'}{m}$. This formula is founded on the "presumption that the moving force, which the body undergoes, and which is "denoted by $m-m'$, is confined to the mass m. But, it must be distributed not "only over the moving body, but on all the particles of the fluid set in motion "by that body; and consequently the denominator of the expression, denoting "the accelerating force, must necessarily be greater than m." M. **Bessel** then enters into a mathematical investigation of the principles from which the results of his experiments are deduced: and at length comes to the following important conclusion: viz. "that a fluid of very small density, surrounding a "pendulum, has no other influence on the duration of the vibrations than that "it diminishes its gravity and increases the moment of inertia. When the in-"crease in the motion of the fluid is proportional to the arc of vibration of the "pendulum, this increase of the moment of inertia is very nearly constant: in "all other cases it will depend on the magnitude of that arc."

The obvious inference from those experiments and researches is, that the amount of the correction will not only vary according to the *length*, *magnitude*, *weight*, *density* and *figure* of the pendulum; but also that in the case of the convertible pendulum (except perhaps in that particular instance when it makes the least number of vibrations possible,) the correction will not be the same for the two knife edges: and consequently that a pendulum, which has been made convertible in air, will no longer be so when tried in a vacuum. It becomes therefore of importance to know how far the differently constructed pendulums, made use of by various experimentalists, are affected by this newly discovered principle, in order that their results may be strictly comparable with each other. The amount of the required correction, however, cannot (according to our present knowledge of the subject,) be determined by calculation, but must, in every case, be ascertained by actual experiment. The most direct method of effecting this appears to be, as M. **Bessel** states (page 37), by swinging the pendulum in a vacuum: although he himself, on account of some doubts which he entertained of this method, but which he has not explained, adopted another and a very different plan.

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* Untersuchungen über die Länge des einfachen Secundenpendels, von F. W. **Bessel**. Berlin 1828, 4to. This work forms part of the Memoirs of the Royal Academy of Sciences of Berlin for 1826.

The mode adopted by M. **Bessel** was of two kinds. The first and principal one was by swinging in air two spheres of equal diameter (about 2·14 inches) but of very different specific gravity, viz. brass and ivory, suspended by a fine steel wire: the other, which was not commenced till the subsequent year, was by swinging the same sphere (brass) first in air and afterwards in water. The result of the experiments, by both these methods, showed that the usual correction for the reduction to a vacuum was much too small; and that the true correction was nearly double what has been generally assumed. The first method gave 1·946*, and the second 1·625, as the *factor* by which the old correction must be multiplied in order to obtain the true correction. These values differ materially from each other; but M. **Bessel** prefers the former, as his investigations are founded on the theory that the vibrations are made in a medium of very small density†.

Being desirous of ascertaining, by a different process, the true value of the correction for the numerous and various pendulums in my possession, as accurately as experiments of this kind will decide the fact, and considering the subject to be otherwise of importance in a scientific point of view, I resolved to devote some time to its examination: and, for this purpose, caused a vacuum apparatus to be fitted up at my own house, where I could pursue the subject at leisure. This vacuum apparatus is very different, in its form and construction, from that which is erected at the Royal Observatory at Greenwich, and described by Captain SABINE in the Philosophical Transactions for 1829, page 207. It consists of a brass cylindrical tube about five feet long and six inches and a half in diameter, rounded at the bottom, and soldered at the top to a thick iron frame, on which the agate planes rest. This frame is firmly screwed and fastened to solid mahogany beams which are securely wedged between two fourteen-inch walls in the corner of a room, which is remarkable for preserving an uniformity of temperature, during the day, throughout the different seasons of the year. The upper surface of this iron frame is ground perfectly plane, and is surmounted with a moveable glass top, in the manner described by Captain SABINE. The brass tube has two small openings, or windows (cut on opposite sides) at a proper distance from the top, which are covered with plate glass, for the purpose of observing the arc of vibration and the coincidences with the clock, which is placed behind. The lower part of the tube is secured also by cross beams, in order to prevent any lateral motion in the tube itself, during the vibrations of the pendulum. As the whole of the experiments about to be related, are *comparative* only, it will be unnecessary to enter more minutely into a description of this apparatus; the form and construction of which will be best understood from the annexed sketch. The flexible metallic pipe, communicating with an air pump, enters the tube immediately under the upper beams: and a brass wire, passing through a stuffing-box, for the purpose of setting off the pendulum at any given arc, enters the tube just below the glass window.

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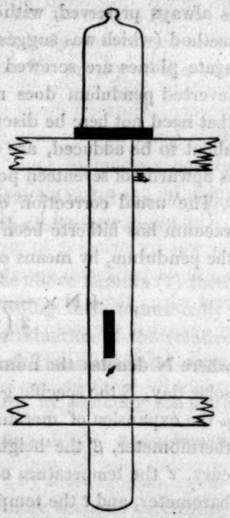
* In a paper, subsequently inserted in the Astronomische Nachrichten, No. 223, M. **Bessel** has increased this value to 1·956.

† M. **Bessel** also swung a hollow brass cylinder alternately in air and in water, and has deduced some results which appear to be astounding: but, I shall show in the sequel that the discordancy in the results stated by him, will be removed by the assumption of a different specific gravity of the moving body, from that which he has adopted.



I ought however here to mention that the agate planes are not, as in Captain SABINE's experiments, *screwed* to the iron frame, but are attached to another solid frame (of brass) three quarters of an inch thick, and having three foot-screws, for the purpose of levelling the planes. These screws merely *rest* on the iron frame: one of them in a conical hole, another in a groove, and the third on the flat surface of the iron frame; by which means, the same position is always preserved, without any strain on the screws. I believe that this method (which was suggested by Mr. TROUGHTON,) is as secure as where the agate planes are screwed to the frame: and the application of Mr. HARDY's inverted pendulum does not detect the least motion. But this is a question that need not here be discussed; since, as I have just stated, the experiments, about to be adduced, are only comparative. The weight of this brass frame is upwards of seventeen pounds and a quarter troy.

The usual correction of the number of vibrations for the reduction to a vacuum has hitherto been deduced from the relative weights of the air and of the pendulum, by means of the following formula*: viz.

$$+ N \times \frac{1}{2\left(\frac{S}{\sigma} - 1\right)} \times \frac{\beta'}{\beta\,[1 + \mu(\tau' - t)]} \times \frac{1}{1 + \alpha(t' - t)}$$

where N denotes the number of vibrations made by the pendulum in a mean solar day, S the specific gravity of the pendulum, σ the specific gravity of air, μ the expansion of mercury, and α the expansion of air for one degree of the thermometer, β' the height of the barometer, τ' the temperature of the mercury, t' the temperature of the air during the experiments, β the height of the barometer, and t the temperature of the air, assumed as standards for the specific gravity.

If we suppose that the temperature of the mercury in the barometer is the same as that of the air surrounding the pendulum, which may generally be assumed as the case, in experiments of this kind, without the risk of any perceptible error, the above formula may be rendered more convenient as follows: viz.

$$+ N \times \frac{1}{2\left(\frac{S}{\sigma} - 1\right)} \times \frac{\beta'}{\beta} \times \frac{1}{1 + (\alpha + \mu)(t' - t)} \qquad (1)$$

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* See M. MATHIEU's paper on this subject, in the Con. des Tems for 1826, page 288.

But, here it will be proper to remark that S does not denote the specific gravity of the pendulum, as determined in the usual manner when *at rest*, unless the mass be homogeneous: for, in all other cases, where the pendulum consists of several parts, whose specific gravities are different, we must compute the *vibrating* specific gravity of the mass in the following manner. Let d', d'', d''', &c. denote the distance of the centre of gravity of each body respectively from the axis of suspension*: w', w'', w''', &c. the weight (in air) of each body: s', s'', s''', &c. the specific gravity of each body, determined in the usual manner. Then will the required *vibrating* specific gravity of the pendulum be†

$$S = \frac{w' d' + w'' d'' + w''' d''' + \&c.}{\frac{w' d'}{s'} + \frac{w'' d''}{s''} + \frac{w''' d'''}{s'''} + \&c.} \qquad (2)$$

And, it is in this manner that I have deduced what may be called the *vibrating specific gravity*, for all those pendulums, which, in the following experiments, consist of substances of different specific gravities.

With respect to the other quantities involved in the above formula (1) there are two modes which have been pursued for expressing them numerically: viz. one by assuming Sir **George Shuckburgh**'s determination of the relative weights of air and water, as stated in the Philosophical Transactions for 1777; that is, $\sigma = \frac{1}{836}$, $\beta = 29{\cdot}27$, and $t = 53°$: and the other by assuming the more recent determination of MM. **Arago** and **Biot**; that is, $\sigma = \frac{1}{770}$, $\beta = 29{\cdot}9218$, and $t = 32°$. The former has been adopted I believe by most English experimentalists; but, as I conceive the latter to be the more accurate determination, I shall adopt it in all the present reductions. They differ from each other about $\frac{1}{93}$th part of the whole correction: the French result being the greatest in amount.

The expansion of mercury is generally assumed equal to ·0001 for each degree of **Fahrenheit**'s thermometer: but the expansion of air is not quite so well agreed upon. It has generally been taken at $\frac{1}{480}$th of its bulk, or ·002083 for each degree of Fahrenheit: but this value applies more particularly to air rendered perfectly *dry* for the purpose of the experiments from which such value has been deduced. The expansion of common atmospheric air, impregnated, as it generally is, with a certain degree of *moisture*, is supposed by M. Laplace to be $\frac{1}{450}$th of its bulk, or ·002222 for each degree*. I have assumed this latter value, and consequently make $(\alpha + \mu) = {\cdot}0023$. Whence the numerical expression for the formula in question will be

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* When the body is *below* the axis, d is *plus*: when *above*, it is *minus*.

† I am indebted to Professor **Airy** for this formula: which, although of considerable importance in all investigations relative to the pendulum, has not, as far as I am aware, been alluded to by any writer on the subject, except **Bessel**.

$$+\mathrm{N} \times \frac{1}{2(\mathrm{S} \times 770 - 1)} \times \frac{\beta}{29{\cdot}9218} \times \frac{1}{1 + {\cdot}0023\,(t' - 32^\circ)} \qquad (3)$$

If we make $\beta = 1$, and $t' = 32^\circ$, we might readily obtain for each pendulum, a *constant quantity*

$$\mathrm{C} = \mathrm{N} \times \frac{{\cdot}0000217016}{\mathrm{S} - {\cdot}001299} \qquad (4)$$

for one inch pressure of the atmosphere, at the freezing point of water; whence the value of the correction at any other pressure β, and at any other temperature t, would be denoted by

$$\mathrm{C} \times \frac{\beta}{1 + {\cdot}0023\,(t - 32^\circ)} \qquad (5)$$

This is the old correction, which is so far erroneous that no account is taken of the effect of the air set in motion by, and accompanying the pendulum, as if *adhering* thereto; and which is now found to influence the result very materially. This formula, however, will still be of considerable service to us, since not only M. Bessel's experiments, but also those about to be detailed in this paper, have for their object the determination of the *factor*, by which the quantity C must be multiplied (according to the form and construction of the pendulum,) in order to produce the *true* correction: this being one of the most convenient forms of showing the relative value and amount of this new influence. I have already stated that the mode proposed to be pursued in the following experiments, for the determination of this *factor*, was to swing the pendulum under the full pressure of the atmosphere and also in a highly rarefied medium, nearly approaching to a vacuum. Let N′ denote the number of vibrations made by a pendulum in a mean solar day (corrected in the usual manner for the rate of the clock, the arc of vibration and the temperature of the room, *but not for the height of the barometer*), β' the height of the barometer, and t' the height of the thermometer when the pendulum is swung under the full pressure of the atmosphere: and let N'', β'', t'' denote respectively the same quantities, when it is swung in a highly rarefied medium. Then will $\frac{N''-N'}{\beta'-\beta''}$ express the true correction for one inch pressure of the atmosphere at the temperature t°; where $t^{\circ}=\frac{1}{2}(t'+t'')$: which, being multiplied by $1+{\cdot}0023\,(t^{\circ}-32^{\circ})$, will give the *true constant*

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* Système du Monde, 5th edition, 1824, page 89. See also Biot's Traité d'Astronomie Physique, vol. iii. (Mésures Barométriques, page 14.)

$$C'=\frac{N''-N'}{\beta'-\beta''}\times\Big[1+{\cdot}0023\,(t^{\circ}-32^{\circ})\Big] \qquad (6)$$

for the same pressure, and at the freezing point: whence we obtain the following expression for the *true* correction, at any pressure β, and at any temperature t: viz.

$$C'\times\frac{\beta}{1+{\cdot}0023\,(t-32^{\circ})} \qquad (7)$$

agreeably to which formula I have deduced the value of C′ in the experiments about to be detailed.

Now, the value of C′ is always greater than C: and, in order to determine the *factor* by which C must be multiplied in order to produce the true correction, (which factor will vary according to the form and construction of the pendulum,) we must make $C'=n\,C$: whence we obtain, for the factor required,

$$n=\frac{C'}{C} \qquad (8)$$

and it is in this manner that the value of the factor (n) has been deduced in the following experiments. And it may be proper to state that the quantity which is here denoted by n, M. Bessel expresses by $(1+k)$.

Description of the Pendulums.

The number of pendulums, for which I have deduced the comparative results, by swinging them in a vacuum apparatus, amounts to forty-one *; varying from each other in figure, dimension, weight, specific gravity, length, mode of suspension, or some other influential property: and comprise almost every variety of pendulum that is ever likely to be made the subject of experiment. In order to prevent confusion in occasionally referring to them, I shall here arrange them in numerical order, and class them according to their form.

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* This number has been more than *doubled* by the experiments hereafter alluded to, made subsequent to the reading of this paper.

No. 1, 2, 3, 4, are spheres of platina, lead, brass and ivory; all of the same diameter; which is somewhat less than 1½ inch. The platina sphere (No. 1.), which has been kindly lent to me, for the occasion, by the Astronomer Royal, is of French manufacture, and about 1·44 inch in diameter; which is the same size as that used by M. Biot in his pendulum experiments, and in fact appears to have been formed from the same model *. It is furnished with a copper *calotte*, and also with a knife edge, attached to a frame, capable of being brought to a state of synchronism with the pendulum with which it is used, by means of a screw, in the manner described by M. Borda in the Base du Systême Métrique, vol. iii. page 338. Its specific gravity I found to be 21·042; and it weighed 8963 grains. The copper calotte weighed 87 grains, and was firmly attached to the platina sphere by means of shell-lac; as the ordinary mode, by greasing the parts, failed in the present experiments. I unfortunately attempted the usual method, in the first instance, not recollecting that the adhesion is caused principally by the pressure of the atmosphere; and that when that pressure is removed the sphere would no longer be supported. This proved to be the case; and the platina sphere, in its fall, received a slight cut against the sides of the vacuum apparatus; but not of sufficient importance to impair its accuracy in any future experiments. It certainly cannot affect the present ones, which are merely comparative. The *vibrating* specific gravity of the mass, including the iron wire to which I shall presently allude, was computed to be 20·745. The leaden sphere (No. 2), the brass sphere (No. 3), and the ivory sphere (No. 4), were ordered to be made of the same size as the platina one: but they are somewhat larger, being 1·46 inch. They have no calotte, but were tapped for the purpose of inserting a brass screw, perforated with a small hole for the insertion of the wire by which they were suspended. The screw weighed 19½ grains: and the same screw has served for all the experiments, where it was required, except for the platina sphere. The wire employed in these and all the subsequent cases, unless otherwise expressed, was of iron about $\frac{1}{70}$th of an inch in diameter; and weighed about 11 grains*: its specific gravity I found to be 7·666. I was unwilling to use a finer wire (except with the ivory sphere), for fear of accidents, the issue of which could not be easily remedied in the vacuum apparatus. In each of these experiments the wire was attached, at its upper end, to the shank (1·55 inch long) of the knife edge, on which the vibrations were made; in the manner described by MM. Borda and Biot: and the adjustment of this knife edge apparatus to a state of synchronism with the pendulum was always attended to. The specific gravity of the leaden sphere including the brass screw I found to be 11·250; and they weighed 4648 grains: of the brass sphere and screw, 7·660; and they weighed 3217 grains: and of the ivory sphere and brass screw, 1·864; and they weighed 776½ grains†; but in all the cases where the pendulum has consisted of more than one metal (or even of two pieces of the same kind of metal, but of two different specific gravities,) the *vibrating* specific gravity of the mass has been deduced from the formula (2). The wire, by which the ivory sphere was suspended, was the finest silver wire that would sustain it with safety; and weighed little more than half a grain. As these three spheres are not of precisely the same diameter, I shall designate them as the 1½ inch spheres.

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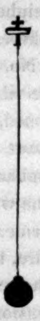
* Base du Systême Métrique, vol. iv. page 449.



No. 5, 6, 7 are spheres of lead (No. 5), brass (No. 6), and ivory (No. 7), all ordered to be made of the same diameter, viz. 2·06 inches; which was intended to be, and is nearly, the same size as the spheres used by M. Bessel‡. These spheres were tapped in the manner already described, for the purpose of inserting the screw above mentioned: and the same knife edge and iron wire, as those above described, were used in all the experiments. The specific gravity of the leaden sphere and the brass screw I found to be 11·281; and they weighed 13019 grains: the specific gravity of the brass sphere and the screw I found to be 7·995; and they weighed 9302 grains: and the specific gravity of the ivory sphere and the brass screw I found to be 1·747; and they weighed 2066½ grains. I shall designate these three spheres, as the 2-inch spheres.

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* As a new piece of wire was occasionally found necessary, I have given what I consider the average weight.

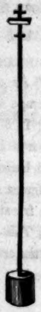
† In obtaining the specific gravities of the different substances, alluded to in this paper, I would observe here, once for all, that I used (not distilled water, but) river water that had been filtered and boiled. The values deduced are the results of two, and sometimes three, different weighings on different days; and are sufficiently accurate for the *comparisons* intended. They are all reduced to the freezing point of water, and to 29·9218 inches of barometric pressure.

‡ This is the exact size of the engraving of the sphere in M. Bessel's work; where it is stated to be the true size: but on subsequently examining the detail of the experiments I found that the correct size is 2·14 inches. The plate had probably shrunk in its dimensions, since it had been printed.

No. 8, 9 are the same leaden and ivory spheres as No. 5 and 7: but the vibrations, instead of being made on the knife-edge above mentioned, were made by causing the wire to pass over a steel cylinder about one fifteenth of an inch in diameter, in a manner somewhat similar to that adopted by M. Bessel in some of his experiments. The wire used with the leaden sphere was the same iron wire as in the former experiments: but that used with the ivory sphere was fine silver wire, rather thicker than that used with No. 4, and weighed 2 grains. The experiments made with these spheres, and with this mode of suspension, are not (I fear) entitled to much credit; for reasons which I will presently explain.

No. 10 is a solid brass cylinder 2·06 inches in diameter and 2·06 inches high; in order to correspond in dimensions with the brass sphere. It was tapped with a screw-hole on its flat side; and was supported by the same iron wire and knife edge as above described. Its specific gravity, with the screw, I found to be 8·174; and they weighed 14190 grains.

No. 11 is the same solid brass cylinder, tapped with a screw-hole on its circular side: but, as it was liable to turn on its axis when suspended by the iron wire, I was obliged to support it with a rod, or piece of thick brass wire, 0·185 inch in diameter, and 37½ long; the upper end of which was attached to the knife edge above mentioned, on which the whole vibrated. The rod weighed 2050 grains, and its specific gravity was somewhat greater than that of the cylinder. The computed specific gravity of the whole was 8·202. This pendulum was swung with its cylindrical side opposed to the line of its motion.



No. 12 is the same solid brass cylinder, supported by the same brass rod, and in the same manner as in the preceding case, except that it was now swung with its flat side opposed to the line of its motion.

No. 13 is the same solid brass cylinder, supported by the same brass rod screwed into its flat side (as in No. 10): an experiment made for the purpose of determining the difference in the results, when suspended by the brass rod, and by the iron wire. See the preceding figure, which exhibits this pendulum.

No. 14 is a cylinder of lead, 2·06 inches in diameter, and 4 inches long; tapped with the screw-hole on its flat side, and supported by the same iron wire and knife edge as above mentioned. It should here be remarked, however, that this cylinder was not wholly of lead; since it was formed of a thin brass tube filled with lead: and this tube was made to slide into an outer cylinder of brass, having the dimensions above described, as will be more fully explained in the next article. The specific gravity of the whole I found to be 10·237; and it weighed 34500 grains.

No. 15, 16, 17, 18 are cylindrical tubes of brass, 2·06 inches in diameter on the outside, 4 inches long, and 0·13 inch thick. These, however, are not different tubes, but consist of one and the same cylindrical outer piece; and is in fact the tube into which the leaden cylinder is made to slide, as mentioned in the preceding article. This cylindrical outer piece is capable of being varied in the four following ways, by means of an inner sliding tube. No. 15 is when both the ends are open, with the exception of a narrow cross piece at the top, to which the screw is attached. No. 16 is when the top is still left open, but the bottom closed. No. 17 is when the top is closed, and the bottom left open. And No. 18 is when both ends are closed. In all the cases, the tube was suspended by the same kind of iron wire as that already described; and from the same knife edge. The specific gravity of the metal I found to be 8·453: but here it may be proper to remark (what I shall again advert to, in the sequel,) that when a *hollow* body is swung as a pendulum, we must take into account the quantity of air contained within the moving body (which, in the present case, is computed to be 3050 grains,) and diminish the specific gravity of the metal accordingly*. Proceeding on this principle, I have calculated the specific gravities of each of these hollow pendulums as follows; to which also I have annexed the weights.

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* Cases of this kind appear to admit of two distinctions: one, where the hollow body is hermetically sealed; the other, where the included air communicates freely with the surrounding atmosphere, and consequently escapes under the action of the air-pump. But, in the case of a cylindrical tube (like that in question) there will be no difference in the result: as, from the similarity of distribution of the masses of metal and of air (at least, in the case of the tube, open at both ends; and ap-

No.	Spec. grav.	Grains.
15 =	2·536	8497
16 =	2·623	8922
17 =	2·561	8622
18 =	2·649	9048

After the experiments with these tubes were completed, I caused the inner sliding tube to be filled with lead, as mentioned in the preceding article: and this solid cylinder could be readily put into and taken out of the outer tube, at pleasure. And when the experiments with this solid cylinder were completed, a new top piece was made to the outer tube, which was closely soldered on: a new bottom piece was also made, to screw on and off, which, by the application of an oiled leather to the screw, might at any time be rendered hermetically sealed.

No. 19 is the tube thus hermetically sealed. The inner sliding tube having been taken away, the weight was reduced to 7250 grains: the specific gravity I found to be 2·233*. The hollow portion of the cylinder now contains 3·255 grains of air.

No. 20 is a lens of lead, 2·06 inches in diameter; 1 inch thick in the middle, and having a flat circumference about a quarter of an inch wide. This lens was tapped with a screw-hole on one of its protuberant sides, and was supported by the same iron wire and knife edge as above described: the position of the lens was consequently horizontal. Its specific gravity with the screw I found to be 11·254; and they weighed 6505 grains.

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proximately so, in the other cases,) the centre of oscillation of the included air will coincide with that of the metal; and the centre of oscillation of the compound mass will therefore coincide with that of the metal alone.

* When the bottom piece of this tube was *loosely* screwed (so as to admit the free passage of the air under the exhausted receiver,) it might be considered as a pendulum similar to No. 18, with the specific gravity of 2·233: and when the bottom piece was wholly taken away, it might be considered as a pendulum similar to No. 17. Experiments were made with the tube under these circumstances, to which I shall allude in the sequel, fully confirming the results of the former ones. In the latter case, when the bottom piece was taken away, the weight was reduced to 6744 grains; and the specific gravity was computed to be 2·042. The solid sliding cylinder is also adapted to this new state of the tube; but at present I have not made use of it, in this way.

No. 21 is a solid copper cylindrical rod, 0·41 inch in diameter, and 58·8 inches long. This pendulum was invented by Mr. TROUGHTON, and was made by him about 16 years ago, when the Commissioners were appointed by Government for determining the length of the seconds pendulum. It would take up some time to describe the mode in which this pendulum was originally intended to be mounted and swung; and would be irrelevant to the present subject: but, as great part of the apparatus could be dispensed with, on the present occasion (the results being comparative only), I shall merely state that I at first attempted to swing it by suspending it, at one end, with a piece of steel wire, drawn close up to the cylinder mentioned in No. 8 and 9. But I found the discordancies (to which I shall afterwards have occasion to allude,) so enormous, that I was obliged to abandon this mode: and I ultimately fastened it, by means of an adjusting screw, to the knife edge used in the preceding experiments. As I had no means of determining the specific gravity of the rod, I have assumed it as equal to that of the copper bar No. 27; viz. 8·629: its weight is 16810 grains.

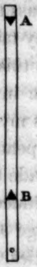
No. 22 is KATER's invariable brass pendulum. Several pendulums of this kind have been made for our own, and for other Governments, and for public bodies; and all from the same model, which is that described by Captain KATER in the Philosophical Transactions for 1819, page 341. I have two now in my possession (numbered 10 and 11,) belonging to the Admiralty; and are those that were taken out by the late lamented Captain FOSTER, in his voyage of experiment. They are formed of a bar of brass 1·8 inch wide, and rather less than $\frac{1}{10}$th of an inch thick. At the top of this bar is a knee piece, also of brass about three tenths of an inch thick, to which a steel knife edge is firmly screwed: and, at about 40$\frac{1}{2}$ inches from this knife edge, is fastened a flat circular bob of solid brass, about 6 inches in diameter, and 1$\frac{1}{4}$ inch thick, but tapered at the edge. Below this bob the bar is reduced to about $\frac{7}{10}$ths of an inch in width, and is continued about 16$\frac{1}{2}$ inches: thus forming what is called a tail piece,—a most unnecessary and inconvenient appendage; since the arc of vibration, which this tail piece was intended to indicate, can be as readily observed by means of the *edge* of the bar above the bob. As my vacuum apparatus was not sufficiently large to receive the bob of this pendulum, I shall deduce the results from the experiments made by Captain

Sabine on two similar pendulums, with the vacuum apparatus at Greenwich; as described by him in the Philosophical Transactions for 1829, page 235. The specific gravity of this pendulum I have assumed equal to 8·4. Captain Kater states that the specific gravity of the first pendulum which he made of this kind was 8·61 (See Philosophical Transactions for 1819, page 354): but this is greater than that of any brass that I have yet found, and greater I believe than what is usually met with; it is even considerably more than the specific gravity of his convertible pendulum, mentioned in the following article, which was formed of nearly similar materials, and which was only 8·248. Captain Sabine, relying on this single experiment of Captain Kater's, has assumed 8·6 as the proper specific gravity for a pendulum of this kind: and as the results therefore which I have deduced from his experiments will not exactly agree with those that he has given, it was necessary here to state the principal cause of the discordancy. I estimate the weight of this pendulum at 90500 grains, from the mean of the weights of two similar pendulums in my possession.

All the pendulums above described can be swung only in one position. I now come to those which are furnished with two or more knife edges; and which are of the kind called *convertible* pendulums. The knife edges of these pendulums (at least, all those hitherto constructed,) are placed at unequal distances from the centre of gravity; and consequently the same pendulum, when swung with that knife edge placed uppermost which is furthest from the centre of gravity, will set in motion a different quantity of air, and, as far as the subject of the present inquiry is concerned, produce a different result, from that which would be produced when the pendulum is swung from the other knife edge. I shall therefore consider these knife edges, which I shall designate respectively A and B, as two separate and independent pendulums: the term A being applied to that knife edge which is the most distant from the centre of gravity, and the term B to the knife edge at the other end of the pendulum.

No. 23, 24 are the two knife edges A and B, of Kater's convertible pendulum, described by him in the Philosophical Transactions for 1818, page 37: the first of these letters designates the pendulum when the great weight is below; and the other, when the great weight is above. This pendulum, having been successively altered by Captain SABINE, furnishes us with four separate and independent results, according to its form when it was swung: 1°. with the wooden tail pieces 17 inches long with which it was originally furnished: 2°. with those wooden tail pieces reduced to the length of 6·4 inches: 3°. with brass tail pieces 7 inches long, instead of the wooden ones: and 4°. without any tail pieces whatever, and moreover deprived of the small sliding weight. In this last case, it was reduced to nearly the same figure and dimensions as the invariable pendulum (No. 22) just described, but without its tail piece. As my vacuum apparatus was not sufficiently large (as already mentioned,) for a pendulum of this kind, I have deduced the results from the experiments made by Captain SABINE with the same pendulum, in the several states above alluded to, as detailed by him in the Philosophical Transactions for 1829, page 331, &c., and for 1831, page 459, &c. With respect to the specific gravities, I must take that of the first case, which was the original construction of the pendulum, as equal to 7·373; which is the value stated by Captain KATER in the Philosophical Transactions for 1819, page 415. But this is the specific gravity of the body when *at rest*, deduced in the usual manner, and not the *vibrating* specific gravity of the mass deduced from formula (2) above given: and as the weights and distances of the several parts from the axis of vibration are not stated, and are now completely destroyed by the alterations in the pendulum, I have no means of ascertaining how far the results might be affected by this view of the subject. As to the second case, where the wooden tail pieces were reduced to 6·4 inches, I have computed the specific gravity (on the assumption that 7·373 in the former case was correct,) as equal to 7·909. With respect to the remaining two cases, as the pendulum here consists wholly of brass, I have computed the specific gravity from the data given by Captain KATER in the Philosophical Transactions for 1818, page 63, and make it equal to 8·248. Captain KATER's result is 8·469; but I apprehend there must be some error in his computation. The weight of the pendulum is somewhere about 66900 grains: but there appears to be some confusion in the weighings. In the Philosophical Transactions for 1818, page 63, the brass parts alone are stated to weigh 9·57 pounds; which, on the presumption that these are avoirdupois pounds, will be equal to 66990 grains troy. But, in the Philosophical Transactions for 1819, page 415, the weight of the whole pendulum, including the wooden tail pieces (which would probably weigh 500 or 600 grains), is stated to be only 66904 grains.

No. 25, 26 are the two knife edges A and B of a convertible pendulum, formed of a plain brass bar, 2 inches wide, ⅜ths of an inch thick, and 62·2 inches long. The form and construction of this pendulum will be best seen from the annexed sketch, which is taken from the description given of the two following ones in the Philosophical Magazine for August 1828, page 137. At 5 inches from one end of the bar is placed one of the knife edges (A), fastened to knee pieces in the usual manner; and at 39·4 inches therefrom is placed the other knife edge (B). The adjustment to synchronism is coarsely effected by filing away from the requisite end: and ultimately to great exactness by means of a small screw inserted at the end B, reduced to a weight proper for such purpose. Its specific gravity, as obtained from a piece of brass, said to be from the same casting, I found to be 8·034; its weight is 121406 grains.

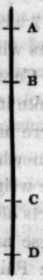


No. 27, 28 are the two knife edges A and B of a copper bar, similar to the last, except that it is ½ an inch thick, and 62·5 inches long. Its specific gravity, as obtained in the manner described in the preceding pendulum, I found to be 8·629: its weight is 155750 grains.

No. 29, 30 are the two knife edges A and B of an iron bar, similar to the copper one, except that it is 62·1 inches long. Its specific gravity, as obtained in the manner described above, I found to be 7·686: its weight is 140547 grains.

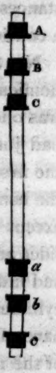
These two last-mentioned pendulums have been already described by me in the Philosophical Magazine as above stated. They belong to the Royal Astronomical Society, and are the same that were taken out by Captain Foster in his late scientific voyage.

No. 31, 32, 33, 34 are the four knife edges A, B, C, D, of a brass bar similar to the three last-mentioned ones, except that it is ¾ths of an inch thick, and 62 inches long. The position of the knife edges will be best understood from the annexed diagram, which is taken from the Philosophical Magazine for February 1829, page 97, where this pendulum is more fully described. It may be sufficient here to



state that the knife edges A and C are rendered synchronous, or nearly so; and that B and D are also rendered synchronous, or nearly so. It follows therefore that each pair (when properly reduced,) should give the same result for the length of the simple pendulum. But the discordancies which they exhibit have been already described by me in the work just quoted, and have given rise to three separate papers on the subject by Captain **Everest**, Mr. **Gompertz** and Mr. **Lubbock** *. The specific gravity of the pendulum, deduced from a piece of metal said to be from the same casting, I found to be 8·060: its weight is 231437 grains.

No. 35, 36, 37, 38 are four of the knife edges, or rather *planes*, A, C, *a*, *c*, of a brass cylindrical tube, or rather tubes, for it is formed of 7 different tubes drawn closely one within the other; so that their joint thickness, which is very firm and compact, and appears as one solid body, is about 0·13 inch. The diameter is 1½ inch on the outside, and it is 56 inches long: the ends are not closed. The specific gravity of the metal I found, by weighing a piece of the tube itself, to be equal to 8·406: but as the included air must be taken into account, the diminished specific gravity of the moving body (deduced agreeably to what is stated in page 411,) will be 3·034. Its weight is 81047 grains. This pendulum is of a totally different construction from any hitherto made: for instead of being fitted up with steel knife edges that vibrate on agate planes, it is furnished with steel planes that vibrate on a pair of agate knife edges which is common to all the planes. The mode of suspension therefore is, in this case, reversed. The pendulum has six planes: but, as two of them (B and *b*) have not yet been used, I shall confine my remarks to the four above enumerated. At the distance of 4 inches from each end of the tube is placed one of the planes, fastened to a brass collar, firmly fixed to the tube. At 12 inches distance from each of these, towards the centre, is placed another plane; thus forming four in the whole.



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* In this last-mentioned paper, which is inserted in the Phil. Trans. for 1830, page 201, Mr. **Lubbock** has shown the effect on the number of vibrations of a given pendulum, corresponding to given deviations in the position of the knife edges. And the result is, that no error of any considerable (or even appretiable) magnitude can arise from such causes, when the artist uses even the most ordinary precaution in fixing the knife edges in their proper position. The discordancies, I believe, arise from irregularities in the knife edge or planes; as I shall more particularly allude to, in the sequel.

The two planes (B and *b*) not here enumerated, lie between the other pairs; as will be best seen in the preceding figure. The vibrations on the planes A and *a*, are rendered synchronous, or nearly so; and also on the planes C and *c*. The length between each synchronous pair of planes is, as nearly as possible, equal to the standard yard.

This completes the list of pendulums hitherto proposed or adopted for the purpose of any physical inquiry, and it embraces almost every variety that has been suggested. I took advantage however of the favourable opportunity that was presented for trying the effect of the pressure of the atmosphere on a few *clock* pendulums. In these cases the pendulum was suspended by a spring, in the same manner as when it is attached to the clock. I shall not stop to inquire whether the arcs, on these occasions, diminished in a geometric ratio; because as the experiments were carried on nearly under the same circumstances in each case, the comparative results will be but little affected by such a consideration.

No. 39 is a mercurial pendulum, such as is now generally attached to astronomical clocks. The pendulum actually employed by me on this occasion, was one that Mr. Hardy was about to attach to an excellent clock which he had just made for His Royal Highness the President of this Society; and is the first that has ever been submitted to so rigid a test. It is constructed in the usual manner, and similar to one described by me on a former occasion *, except that the rod and sides of the stirrup are half an inch wide, which I consider an improvement. The whole is rivetted together in a very firm manner, and finished in a very superior style. The height of the mercury in the glass cylinder, when I swung it, was 6·8 inches. The vibrations were made, as I have already observed, on its own spring, and not on a knife edge. The weight of the mercury was 82960 grains, the weight of the glass cylinder was 6463 grains, and the weight of the steel parts was 13565 grains. The specific gravity of the glass I found to be 3·300; and I have assumed that of mercury to be 13·586, and of steel to be 7·800: the *vibrating* specific gravity, therefore, of the mass, deduced agreeably to the formula (2), I find to be 10·591.

No. 40 is another clock pendulum formed of a cylindrical rod of deal, about ⅜ths of an inch in diameter, passing (at its lower end) through a cylinder of lead

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* Memoirs of the Astronomical Society of London, vol. i. p. 409.

1·8 inch in diameter, and 13·5 inches long; in the manner described by me in the paper just alluded to. The specific gravity of the lead I have assumed as equal to 11·300: but on account of the cylindrical hole made in it, and the wooden rod inserted therein, I estimate the *vibrating* specific gravity of the mass as equal to 11·113 only.

No. 41 is the same cylinder of lead attached to a flat rod of deal, 1 inch in width, and about 0·14 inch thick in the middle of its width, but bevilled to a thin edge. The cylindrical hole was (as in the preceding case,) completely filled with the rod, which was designedly constructed in that form at its lower end, in order to exclude the air which would otherwise remain in the cylinder and thus alter its specific gravity. The *vibrating* specific gravity of the mass is therefore the same as the preceding: and it was also suspended by the same spring. It was swung with its thin edge opposed to the line of motion. The weight of the leaden cylinder is 93844 grains.

Results of the Experiments.

Having thus given a description of the several pendulums employed in the following experiments, I shall now proceed to state the results obtained from each of them respectively: dividing them into different sets according to the form and construction of the pendulum. And here I would remark that the number annexed to each result denotes the number of the experiment, as given in numerical order in the *Appendix* to this paper; where all the necessary information for obtaining the result, is given in detail: this mode of reference being considered preferable to an interruption of the narrative in this part of the paper. I would also previously observe that, in conducting these comparative experiments, I have always made them *in pairs*, on the *same day*, and *immediately* succeeding each other; whereby any discordancy arising from an alteration of temperature of the room, or the rate of the clock, is in a great measure avoided: and, in continuing any series, the order of proceeding has been alternately reversed, which is an additional check against any error arising from a *progressive* (but unperceived and consequently unrecorded) variation in the rate of the clock, or the temperature of the room. Thus, when four experiments have been successively made (which is the smallest number employed,) I have swung the pendulum first in free air; then, after pumping out the air and the lapse of a given interval (for the equalization of the temperature which is always disturbed by this process,) but without touching any part of the apparatus, I have taken the second series in vacuo: these two sets form one comparison. On the conclusion of this series (everything remaining the same, and no part of the apparatus being in any way disturbed or handled,) I have immediately taken the third series in vacuo: then, after letting in the air, and suffering everything to remain undisturbed as before for a given time, I have taken the fourth series in free air; which is compared with the experiment immediately preceding, and thus forms a second comparison. These four experiments, thus compared, give two results, which are in general sufficient for the determination of the quantity sought. But, I have frequently repeated the process and taken four other consecutive sets: in which case I have usually taken off the glass top, and turned the knife edge, end for end, for a reason which I shall more particularly allude to in the sequel; and have then conducted the new series precisely in the same manner as the former ones. The pendulum has generally been set off, as nearly as possible, to the same arc of vibration, and continued for nearly the same length of time. In short, I have endeavoured, as much as was in my power, to make each *pair* of experiments which are compared together, as nearly as possible under the *same* circumstances, in order to avoid the chance of any error or discordancy arising from any unforeseen cause*.

First set.—*Results with the 1½-inch Spheres.*

1) Platina.		2) Lead.		3) Brass.		4) Ivory.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
1—2	1·873	17—18	1·896	9—10	1·819	25—26	1·879
3—4	1·883	19—20	1·909	11—12	1·817	27—28	1·864
5—6	1·866	21—22	1·840	13—14	1·849	29—30	1·858
7—8	1·904	23—24	1·840	15—16	1·849	31—32	1·886
Mean =	1·881	Mean =	1·871	Mean =	1·834	Mean =	1·872

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* These remarks apply more especially to the experiments recently made for the express purpose of this inquiry. Other experiments, made prior to the present year, without reference to this subject, are taken from my observation books, in the order in which they were made.

The results of all these pendulums agree very well together, except the brass one: and seem to show that in pendulums of equal length and of similar construction, the factor for this additional correction depends on the *form* and *magnitude* of the moving body; and is not affected by its weight or specific gravity. The mean of the whole makes n = 1·864*. I am unable to account for the discordancy of the brass sphere from the others; unless it be in the determination of the specific gravity, which is certainly less than that of any brass I have yet examined: it being only 7·660 from a mean of three different weighings on three different days, and agreeing very well with each other. If the specific gravity be assumed equal to 7·8 or 7·9 (which is still small,) the result of this pendulum would agree with the others: but I could never make it exceed 7·67†.

Second set.—*Results with the 2-inch Spheres.*

On the Knife edge.						On the Cylinder.			
5) Lead.		6) Brass.		7) Ivory.		8) Lead.		9) Ivory.	
Exp.	n	Exp.	n	Exp.	n	Exp.	n	Exp.	n
49—50	1·764	33—34	1·736	41—42	1·752	61—62	1·811	57—58	1·760
51—52	1·732	35—36	1·732	43—44	1·759	63—64	1·682	59—60	1·722
53—54	1·717	37—38	1·770	45—46	1·762				
55—56	1·739	39—40	1·767	47—48	1·748				
Mean =	1·738	Mean =	1·751	Mean =	1·755	Mean =	1·746	Mean =	1·741

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* There are some singular coincidences and discordancies in these results which though slight are worthy of notice. For instance, in the experiments with the lead sphere, No. 17—20 are almost identical; and so likewise are No. 21—24, yet differing from the former. Also in the experiments with the brass sphere, No. 9—12 are almost identical; and so likewise are No. 13—16, yet differing from the former. These and other cases of a like kind are trifling anomalies for which I cannot give any satisfactory explanation.

† Some persons have supposed that if the ball be *greased*, the results might be affected: and if so, the present discordancy may have arisen from some accidental circumstance of this kind. It is probable also that the results may vary according to the state of moisture or dryness of the atmosphere; or from some other unknown cause. On these points, there is certainly a wide field of inquiry open, but on which, at present, I have not leisure to enter. The true cause however, of the present discordancy, I suspect to arise from some internal cavities in the sphere (indicated by the smallness of its specific gravity,) which are connected with the screw-hole, and thus suffer the escape of the included air when submitted to the action of the air pump. This contingency cannot be allowed for, in the computation; although it may be appreciable in the result.

If we reject the two results from the cylinder, (which I shall show, in the sequel, cannot be depended upon,) we shall have the mean of the rest equal to 1·748: thus confirming the remark just made, that the factor for this additional correction, in pendulums of equal length and of similar construction, seems to depend on the form and magnitude of the moving body, and is not affected by its weight or density. This result certainly does not accord with that deduced by M. Bessel, from his experiments with brass and ivory balls of nearly the same size as the present ones; which result I have already stated to be 1·946*. M. Bessel's experiments appear to have been conducted with very great care, and with all that accuracy and all those powerful talents for which he is so highly distinguished. At the same time however I would remark that I have carefully revised all my own experiments, and have not been able to discover any source of error: in fact, the general result is corroborated by the uniformity in the results of the experiments with the other pendulums. The subject therefore is still open for further elucidation. In all M. Bessel's experiments, he used wires of two different lengths; one being about the length of the seconds pendulum, and the other differing from it the exact length of the French toise: or, in round numbers, about 39 inches and 116 inches. The value of the factor which he has deduced, appears to be that which he considers common to both: but it perhaps may be a question whether pendulums, differing so much in their lengths, give precisely the same value for the factor.

Third set.—*Results with the 2-inch solid Brass Cylinder.*

Flat sides horizontal.				Flat sides vertical.			
10) Suspended by an iron wire.		13) Suspended by a brass rod.		11) Round side opposed to the line of motion.		12) Flat sides opposed to the line of motion.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
65—66	1·839	77—78	1·905	69—70	1·912	73—74	1·954
67—68	1·880	79—80	1·940	71—72	1·928	75—76	1·946
Mean =	1·860	Mean =	1·922	Mean =	1·920	Mean =	1·950

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* See the note, in page 402.

The difference between the results of the pendulums 10 and 13 will show the effect produced by the substitution of the brass rod for the iron wire*. The results of the pendulums 11 and 13 are, as might have been anticipated, nearly equal. The comparison of the results of the pendulums 11 and 12 will show the difference produced, according to the manner in which the cylinder is swung. The whole appear very consistent with the assumption that in pendulums of equal length and of similar construction, the factor for the additional correction depends on the form and magnitude of the moving body.

Fourth set.—*Results with the 4-inch Cylinder.*

Solid.		Hollow.									
14) Filled with Lead.		15) Both ends open.		16) Top open, bottom closed.		17) Top closed, bottom open.		18) Both ends closed.		19) Hermetically sealed.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
97— 98	2·011	85—86	1·921	89—90	1·937	93—94	1·983	81—82	1·995	101—102	2·055
99—100	2·052	87—88	1·929	91—92	1·943	95—96	1·968	83—84	2·006	103—104	2·085
Mean =	2·032	Mean =	1·925	Mean =	1·940	Mean =	1·975†	Mean =	2·000†	Mean =	2·070†

It appears from these last experiments that the effect of the circumambient air on the moving pendulum is the same whether a portion of the pendulum be solid or hollow; provided we take into account (in the case of hollow bodies,) the diminution of the specific gravity of the pendulum, by reason of

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* It might reasonably be inferred, from this insulated comparison, that the thicker the suspending rod, or wire, the greater would be the value of *n*. But, it will be seen from some *additional* experiments, made since this paper was read and which will be given in the sequel, that this is not always the case: and that the present results can be satisfactorily accounted for, on a totally different assumption.

† The experiments with the top closed and bottom open, and with both ends closed (similar to those of pendulums No. 17 and 18), were repeated after the inner sliding tube had been taken away, and a new top to the outer cylinder had been soldered on, as mentioned in page 412: and the results were as follow:

Top closed, and bottom open.	Both ends closed.
1·977	2·111
1·963	2·094
Mean = 1·970	Mean = 2·102

which agree very well with the preceding results.

I will also take occasion here to observe that, having reason to suspect the escape of the air from the

the included air: and there is little or no difference whether the hollow body be hermetically sealed, or whether the ends be loosely closed, and a free communication left between the internal and external air: due regard being had, in all these cases, to the correct determination of the *vibrating* specific gravity of the body. When both ends of the cylinder are left open, the effect of the air appears to be the least, as in the pendulum 15; and it is increased when either the top or bottom pieces are replaced, as in pendulums 16 and 17: which seems to show that some slight modification of the results is caused by leaving the ends *open* to the circumambient air. I would observe that, with the exception of No. 14 and 19, the specific gravities of the cylinders could not be practically determined, but were computed only; and from assumptions relative to the contents of the cylinder, which could not be completely verified. But they are probably very near the truth; and the comparative results cannot be materially affected by any error that is likely to have occurred. The repetition of three of the experiments, as stated in the preceding note, after the cylinder had been altered, and its contents subjected to a new computation, shows the degree of accordance that may be attained in these experiments.

If the results of the experiments with these hollow cylinders be compared with those made by M. Bessel, with a hollow brass cylinder of a somewhat similar form, vibrating in air and in water, there will be found a very considerable and remarkable difference; inasmuch as he makes the value of n equal to 9·100 *. But, on examining the steps of the process by which he deduces this value, it will be easy to discover the source of this apparent discordancy. The specific gravity of the brass, of which the cylinder was formed, is stated to have been 8·3; but by reason of the included air, the specific gra-

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interior of the cylinder, in the experiment with pendulum 19, when the vacuum tube was exhausted, I repeated the experiment, and found the following result:

$$\begin{array}{r} 2{\cdot}076 \\ 2{\cdot}160 \\ \hline \text{Mean} = 2{\cdot}118 \end{array}$$

But, here also, from some appearances round the screw of the bottom piece, I had again reason to suspect the escape of some of the air from the interior of the cylinder; which may perhaps account for the slight discordancies apparent in the partial results. The whole however are very satisfactory.

* See his work, page 67. He makes the value of k, from two experiments, equal to 7·99 and 8·21; mean = 8·100: to which, unity must be added, in order to obtain the value of n. The diameter of M. Bessel's cylinder was 2·84 inches, and its height 3·20 inches.

vity of the moving mass was reduced to 2·079: and this is the value which M. **Bessel** employs, in deducing the results from the first set of experiments, where he swings the cylinder (closed) first in air and afterwards in water: which result gives $n = 1{\cdot}754$. In the second set of experiments, he takes away the bottom piece of the cylinder, and having swung it first in air (where the diminished specific gravity was nearly the same as before), then immerses it in water, whereby, he says "the specific gravity of the brass, about 8·3, "is restored." With this assumed specific gravity, the value given by M. **Bessel** is certainly correct. But if we suppose that the specific gravity of the moving mass is not restored to the specific gravity of the metal by suffering the tube to be filled with water; and that the pendulum can be considered in no other light than as consisting of a cylinder filled with water, instead of a cylinder filled with air; (the specific gravity of which, instead of being 8·3, will probably not be so much as 2·8;) the value of the result will be materially altered. In fact, if the specific gravity were only 2·5, the value of n would be only 1·85: which differs very little from the value deduced by M. **Bessel** from the experiments with the *closed* cylinder. Now, I find (from the data furnished by M. **Bessel**,) that the specific gravity of the cylinder when filled with water and with the bottom piece annexed, is about 2·8: but it is evident that, when the bottom piece is taken away, the specific gravity will not be so much; and by the assumption of such diminished specific gravity, the discordancy noticed by M. **Bessel**, would be considerably reduced, if not wholly eliminated *.

Fifth set.—*Results with the 2-inch Leaden Lens.*

No. 20.

Exp.	n
105—106	1·614
107—108	1·546
Mean =	1·580

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* M. **Bessel** remarks that in this experiment there was a more than usual motion of the water, arising from a portion of the fluid flowing out of the cylinder to supply the vacuum caused by the motion of the cylinder; and the reverse. But the effect of this on the general result would, I apprehend, be very slight. In my experiments with hollow cylinders, above detailed, we observe but a trifling difference when the ends of the cylinder are left *open*.

The whole of the experiments with the preceding 20 pendulums were made for the purpose of determining the additional correction due to bodies suspended by a fine wire, or by a very thin rod: this being one of the forms in which pendulums are constructed for the purposes of physical inquiry. In the present experiments, the pendulums were all nearly of the same length, or about 39 inches; and the results tend to show that the value of *n*, in pendulums of equal length and of similar construction, depends entirely on the external form and magnitude of the pendulum, and is uninfluenced by its weight or specific gravity. But, whether any portion of this result (and, if any, how much of it,) is to be attributed to the wire or suspending rod; or whether it is caused wholly by the sphere or cylinder; or whether the effect would be greater or less with longer or shorter pendulums; or in what ratio they would be affected by such alterations,—must be left to be determined by future experiments, undertaken with a view to such special investigations *.

I come now to pendulums of a totally different construction.

Sixth set.—*Results with the Copper Cylindrical Rod* 0·41 *inch in diameter, and* 58·8 *inches long.*

No. 21.

Exp.	*n*
109—110	2·952
111—112	2·913
Mean =	2·932

The factor arising from this pendulum is the greatest of any I have yet found: it exceeds all the preceding ones deduced from spheres and cylinders suspended by wires or fine rods; and also the massy bar pendulum No. 31—34, which is 2 inches wide, ¾ of an inch thick, and 62 inches long.

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* Since this paper was read before the Society, I have made several experiments to determine some of the points here alluded to; which, by permission of the Council, are added to this paper, and will be given in the sequel. They tend to open a new view of the subject; inasmuch as they show that, in pendulums suspended in the manner mentioned in the text, the value of the factor *n* is affected not only by the magnitude of the sphere or cylinder, but also by the magnitude and length of the rod or wire.

Seventh set.—*Results with* Kater's *Invariable and Convertible Pendulums.*

	Invariable.		Convertible.				
	22)		23) Knife edge A or heaviest end below.		24) Knife edge B or heaviest end above.		
	Exp.	n	Exp.	n	Exp.	n	
No. 12.	I.	1·588	Ph. Trans. 1829	2·144	Ph. Trans. 1829	2·204	with wooden tail pieces.
	II.	1·589					
No. 13.	III.	1·570		1·840		2·205	with do. reduced.
	IV.	1·574					
	V.	1·615		1·853		2·012	with brass tail pieces.
	VI.	1·606					
			1831	1·811	1831	2·109	without any tail pieces.
				1·910		2·161	
				1·905			
	Mean =	1·590	Mean =	1·875	Mean =	2·135	

The mean result of the invariable pendulum differs from that deduced by Captain Sabine, who makes $n = 1·655$. This difference arises from two causes: in the first place, he adopts Sir George Shuckburgh's determination of the relative weights of air and water; whereas I have preferred, in all these reductions, the more recent determinations of MM. Arago and Biot: and secondly, (which is the principal cause of the difference,) he has assumed the specific gravity of the pendulum equal to 8·600; whereas I do not consider that it can be correctly assumed greater than 8·400, as I have already stated in page 414. Captain Sabine made use of two different pendulums, marked No. 12 and 13; and the results of each accord very well together.

With respect to the convertible pendulum, it is clear that the first determination of the values of n (viz. 2·144 and 2·204,) must be used with all those experiments made by Captain Kater for determining the length of the seconds pendulum, and inserted in the Philosophical Transactions for 1818: subject however to the proper correction for the *vibrating* specific gravity. The last three for the knife edge A, and the last two for the knife edge B, can be applied only to the pendulum as it now exists, deprived altogether of the tail pieces, and its sliding weight.

The detail of the experiments with the invariable pendulum will be found in the Philosophical Transactions for 1829; and of the convertible pendulum, in the same work for 1829 and 1831.

Eighth set.—*Results with a Brass Bar, 2 inches wide, $\frac{3}{8}$ inch thick, and 62·2 inches long.*

25) Knife edge A.		26) Knife edge B.	
Exp.	*n*	Exp.	*n*
113—114	1·872	115—116	2·027
119—121	1·819	117—118	2·007
122—124	1·844	126—128	1·975
124—125	1·865	128—130	1·956
136—139	1·848	131—133	1·945
139—141	1·838	133—135	1·896
Mean =	1·848	Mean =	1·968

Ninth set.—*Results with Copper and Iron Bars, 2 inches wide and ½ inch thick.*

Copper, 62·5 inches long.				Iron, 62·1 inches long.			
27) Knife edge A.		28) Knife edge B.		29) Knife edge A.		30) Knife edge B.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
	1·896		1·998		1·935		2·098
	1·915		1·994		1·926		2·019
	1·856		1·978		1·975		2·078
	1·899		1·994		1·945		2·061
Mean=	1·891	Mean =	1·991	Mean=	1·945	Mean=	2·064

As these two bars are both of the same thickness, it would appear that the shorter pendulum gives the greatest value of *n*: but the discordance probably arises from some error in the assumed specific gravity of the metals; since, as I have already observed, it was not deduced from the pendulum itself. I have not here given the references to these experiments, as the details of them will form the subject of a Report to be laid before Government, which I am about to draw up, relative to the pendulums employed by the late Captain Foster, in his voyage of experiment.

Tenth set.—*Results with a Brass Bar,* 2 *inches wide,* ¾ *inch thick, and* 62 *inches long.*

31) Knife edge A.		32) Knife edge B.		33) Knife edge C.		34) Knife edge D.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
142—143	2·061	145—146	2·071	151—152	2·098	148—149	2·090
143—144	2·057	146—147	2·061	152—153	2·064	149—150	2·046
154—155	2·054	157—159	2·053	164—165	2·111	161—162	2·104
155—156	2·114	159—160	2·127	165—166	2·124	162—163	2·109
Mean =	2·071	Mean =	2·078	Mean =	2·099	Mean =	2·087

If we take the mean of the two knife edges A and D (which are situated at the ends of the bar, and in which positions of the pendulum the heaviest weight is below the axis of suspension,) the value of *n* will be 2·079; and the mean of the other two knife edges B and C, in the reversed positions of the pendulum, will make *n* equal to 2·088: which two values will be the correct mean for this pendulum. But the difference in these values is so trifling, that the general mean ($n = 2·083$) may be assumed for all the knife edges, without the risk of any material error.

Eleventh set.—*Results with a Brass Tube,* 1½ *inch in diameter, and* 56·2 *inches long.*

35) Plane A.		36) Plane C.		37) Plane *a*.		38) Plane *c*.	
Exp.	*n*	Exp.	*n*	Exp.	*n*	Exp.	*n*
169—170	2·318	171—172	2·269	173—174	2·243	167—168	2·293
175—178	2·318	179—180	2·247	181—184	2·291	185—188	2·341
Mean =	2·318	Mean =	2·258	Mean =	2·267	Mean =	2·317

If, as in the case of the preceding bar, we take the mean of the two planes A and *c*, which are situated at the ends of the tube, the values of *n* will be identical with each other, or 2·318: and the mean of the other two planes, in the reversed positions of the pendulum, will make *n* equal to 2·262. So that, with this pendulum the value of *n*, when the heaviest end is above the axis of suspension, is *less* than it is when the pendulum is in the reversed position:

contrary to what takes place with all the preceding convertible pendulums; and contrary to the theory on this subject recently developed by some excellent mathematicians.

Twelfth set.—*Results with Clock pendulums, suspended by springs.*

39) Mercurial.		Leaden cylindrical bob.			
		40) Cylindrical rod.		41) Flat rod.	
Exp.	*n*	Exp.	*n*	Exp.	*n*
189—190	2·441	201—202	2·562	197—198	2·794
191—192	2·316	203—204	2·616	199—200	2·860
193—194	2·350				
195—196	2·267				
Mean =	2·343	Mean =	2·589	Mean =	2·827

Besides these clock pendulums there is another kind, not here enumerated, consisting of a lenticular shaped bob, of some heavy metal, suspended either by a single rod, or by several compensation rods; in which latter case, it is called the gridiron pendulum. As my vacuum apparatus was not sufficiently large to receive a pendulum of this kind, I cannot throw any light on the probable value of *n* in these cases. But, as the bob of such a pendulum is not much unlike the convertible pendulum of Captain KATER, when deprived of its tail pieces, (see the description of the pendulum No. 23 in this arrangement,) we may form some estimate of the probable value of *n*, when the pendulum is suspended by a single rod. In the case of the gridiron pendulum however, it may be a matter of doubt whether the air between the vertical rods may not diminish their specific gravity, when considered as a *vibrating* body. With respect to the leaden cylinders attached to the wooden rods, it will be seen from the experiments with the pendulums No. 40 and 41, that in the case of the thin flat rod, the factor is greater than with the cylindrical rod: contrary perhaps to what might have been anticipated*.

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* This is confirmed by a repetition of the experiment with the same cylinder, and with rods of the same form as the present ones, but of different materials. The difference was, as in the present case, about ·200.

General view of the preceding Results.

Having thus given the detail of the several experiments, I shall bring the mean results of the whole into one general view, in the following Table: where I shall first give the value of the old correction, for the reduction to a vacuum, for each pendulum, on the assumption that the barometer stands at 30 inches, that the thermometer is at 32°, and that the number of vibrations in a mean solar day is in each case exactly 86400; then the value of n, or the factor by which such correction must be multiplied in order to obtain the new correction; which new correction, as deduced from the preceding experiments, is given in the next column. To which I have added, in the last column, the weight of air adhering to and dragged by the pendulum in consequence of the air put in motion thereby, when vibrating in the mean state of the atmosphere above mentioned: or rather the quantity of air which, if applied to the *centre of gyration* of the pendulum, would produce the retardation shown by the experiment. This view of the subject was suggested by Professor AIRY; who, at the same time, favoured me with the following investigation and formula for the computation of the *weight of adhesive air* required.

"Let N denote the number of vibrations made by a pendulum, in a mean "solar day, when swung in air: and let ν be the additional number which it "makes when swung in vacuo. Also let w be the weight of the pendulum, "in grains troy; S its vibrating specific gravity, and σ the specific gravity of "the air. Now, since the force of gravity diminishes in the ratio of $(N+\nu)^2$ "to N^2, or in the ratio nearly of $\left(1+\frac{2\nu}{N}\right)$ to 1, it follows that when the pen-"dulum vibrates in air, it is as if, retaining the inertia of its weight w, it "had the gravity of only $w \times \frac{N^2}{(N+\nu)^2} = w\left(1-\frac{2\nu}{N}\right)$ nearly: or, as if it had "lost the weight $w \times \frac{2\nu}{N}$. But, the weight which it has really lost from the "displacement of a quantity of air is $w \times \frac{\sigma}{S}$. Consequently the portion "which is not accounted for by the mere displacement of the air, is

$$w\left(\frac{2\nu}{N}-\frac{\sigma}{S}\right) \tag{9}$$

" and which may be considered as the additional weight gained by the pendulum, (or rather, the addition to its inertia) when moving in air, supposed " to be *applied to the centre of gyration* *." This is the value given in the last column of the following Table†. Its weight is expressed in grains troy; and the air is supposed to be reduced to the temperature of 32°, and to the pressure of 29·9218 inches: and its specific gravity is assumed equal to ·001299.

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* " The inertia of the whole pendulum, in resisting angular motion, is the same as if it were " collected at the *centre of gyration*. The immediate result of the experiment and formula above " given is, that the inertia of the whole pendulum ought to be increased in the proportion of 1 to " $\left(1+\frac{2\nu}{N}-\frac{\nu}{S}\right)$: or that, instead of supposing the inertia w applied at the centre of gyration, the " inertia $w\left(1+\frac{2\nu}{N}-\frac{\nu}{S}\right)$ ought to be applied there. The addition to the inertia is therefore " $w\left(\frac{2\nu}{N}-\frac{\nu}{S}\right)$, applied where that of the whole pendulum may be supposed to be applied; that is, " at the *centre of gyration*."

† In all the computations, however, instead of using the approximate value $\frac{2\nu}{N}$, I have taken the correct value $(N+\nu)^2-N^2$. The difference is unimportant, unless the specific gravity of the pendulum be very small.

A comparison of the Old and New Corrections, for the reduction to a vacuum: with the Factor by which the former must be multiplied, in order to produce the latter: also the Weight of adhesive air, dragged by the pendulum.

Pendulums.	No.	Old correction.	Factor n	New correction.	Weight of adhesive air.
		″		″	Grains.
Spheres, 1½-inch diameter …. Platina	1	2·709	1·881	5·104	0·496
Lead..........	2	5·003	1·871	9·362	0·468
Brass	3	7·343	1·834	13·467	0·457
Ivory	4	30·080	1·872	56·310	0·472
Spheres, 2-inch diameter, on knife edge, Lead	5	4·988	1·738	8·668	1·115
Brass	6	7·032	1·751	12·317	1·140
Ivory	7	32·143	1·755	56·420	1·164
on cylinder .. Lead	8	4·988	1·746	doubtful cases.	
Ivory	9	32·143	1·741		
2-inch Brass cylinder, with wire, flat sides horizontal	10	6·882	1·860	12·800	1·945
with rod, flat sides vertical *	11	6·859	1·920	13·169	2·378
flat sides vertical †	12	6·859	1·950	13·377	2·451
flat sides horizontal....	13	6·859	1·922	13·188	2·382
4-inch Brass cylinder, solid, filled with lead	14	5·448	2·032	11·070	4·558
hollow, both ends open	15	22·172	1·925	42·686	4·045
top open, bottom closed....	16	21·437	1·940	41·582	4·165
top closed, bottom open....	17	21·955	1·975	43·378	4·283
both ends closed	18	21·227	2·000	42·468	4·454
hermetically sealed	19	25·191	2·070	52·150	4·532
Lens, one inch thick, Lead	20	5·000	1·580	7·900	0·438
Long cylindrical rod, Copper	21	6·519	2·932	19·117	4·904
KATER's Invariable, Brass	22	6·697	1·590	10·649	8·339
KATER's Convertible, with the wooden tail pieces ‡ .... knife edge A	23	7·630	2·144	16·356	
knife edge B	24	7·630	2·204	16·815	
Long Bars 2 inches wide, ⅛ inch thick, Brass, knife edge A	25	7·002	1·848	12·938	16·705
knife edge B	26	7·002	1·968	13·780	19·049
½ inch thick, Copper, knife edge A	27	6·519	1·891	12·330	20·986
knife edge B	28	6·519	1·991	12·980	23·276
Iron, knife edge A	29	7·319	1·945	14·237	22·455
knife edge B	30	7·319	2·064	15·107	25·435
¾ inch thick, Brass, knife edge A	31	6·980	2·071	14·460	40·594
knife edge B	32	6·980	2·078	14·569	
knife edge C	33	6·980	2·099	14·506	
knife edge D	34	6·980	2·087	14·612	
Long Brass tube .... plane A	35	18·546	2·318	42·990	45·937
plane C	36	18·546	2·258	41·874	43·563
plane *a*	37	18·546	2·267	42·048	44·195
plane *c*	38	18·546	2·317	42·974	45·900
Clock pendulums on springs, Mercurial....................	39	5·312	2·343	12·448	17·003
Leaden bob, cylindrical rod........	40	5·190	2·589	13·104	17·462
flat rod..............	41	5·190	2·827	14·312	20·120

* Cylindrical side opposed to the line of motion. † Flat sides opposed to the line of motion.

‡ For the other cases of Captain KATER's convertible pendulum, see page 427.

It appears from this Table that, in the case of spheres, whose diameters are rather less than $1\frac{1}{2}$ inch (which is about the size of that used by M. **Borda**, and by M. **Biot**, in their experiments on the length of the seconds pendulum), suspended by a fine wire, the value of n may in pendulums of such length be assumed equal to 1·86: but that, if the diameter of the sphere be increased to about 2 inches, as in M. **Bessel**'s experiments, the value of n will be diminished to 1·75. I regret that my vacuum apparatus is so constructed that it will not admit of my making experiments on either larger or smaller spheres or on longer or shorter pendulums: otherwise I should have pursued this inquiry further, in order to discover the law by which the results of pendulums so constructed are governed*. It will be seen likewise, from a comparison of the pendulums No. 10 and 13, that the *size* of the suspending wire, or rod, has a perceptible (although in those particular cases, not a very material) effect on the results: increasing the value of n, as the size of the wire increases. The value of n is affected also by the *form* of the rod, as may be seen by a comparison of No. 40 and 41, to which I shall again presently allude.

The solid cylinder, 2 inches long, gives the value of n equal to 1·86†; another, of the same diameter, and double the length, gives 2·03; and the cylindrical tube, 56 inches long, gives only about 2·3: whilst the small cylindrical rod, not much more than 4 tenths of an inch in diameter, gives upwards of 2·9. Other apparent anomalies will present themselves, on a more minute examination and comparison of the values given in the Table; which can only be cleared up by future experiments.

It appears also from this Table that the additional number of vibrations to be applied to the results from experiments with a platina sphere, similar to that made use of by M. **Biot**‡, will be 2·395: whereas the additional number to be applied to the results from the experiments with Captain Kater's convertible pendulum (knife edge A) will, on the assumption that the specific gravity as taken by him is correct, be 8·726 (See page 415). So that these two pendulums, which were considered to be nearly in accordance when the old correction was applied for the reduction to a vacuum, will now differ 6·331 vibrations in a mean solar day, from each other: or $\frac{1}{173}$th of an inch in the length of the seconds pendulum. In each of these computations the pendulum is assumed as making exactly 86400 vibrations in a mean solar day.

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* I have made some alterations in my pendulum apparatus, since this paper was read, which has enabled me to extend the scale of my experiments; as I shall subsequently state more at length.

† Since this paper was read before the Society, I have seen the account of M. **Bessel**'s additional experiments on the pendulum, in the Ast. Nach. No. 223. From those experiments, M. **Bessel** deduces the value of n, for a cylinder very similar to that mentioned in the text, equal to 1·755. In this experiment the length of the wire was nearly the same as mine. But, for his *long* pendulum, he makes the value of n equal to 1·952. He has also slightly increased the value of n as adduced from his former experiments; making it equal to 1·956, instead of 1·946, as already mentioned in page 402.

‡ Although this is the value to be applied to the pendulum used by M. **Biot**, it does not follow that it would be correct to apply the same value to that used by M. **Borda** (which was a *two seconds* pendulum), unless it should be found that the factor is the same for long and short pendulums of this construction.

It appears, from the general Table of comparisons above given, that the long cylindrical copper rod (No. 21) is the most affected by this newly discovered principle; even more so than any of the spheres or cylinders suspended by wires, or than the thick brass bar (No. 31) which presents a flat surface of ¾ of an inch in width, to the line of motion. We find also that the small spheres are more sensibly affected than the larger ones; which agrees with what M. Du Buat observed in the experiments made by him, to which I shall presently allude. But the relation between the results of the other pendulums, does not appear to me, at present to be satisfactorily accounted for, or to be referable to any known principle; and, in order to determine the effect which is produced in the results, they must in all cases be made the subject of actual experiment. We may however draw this inference from the whole, that we cannot strictly compare the results of any invariable pendulums, that have been swung in various parts of the globe, without subjecting them (or their prototypes) to this rigid test. The English pendulums have generally been made after one fashion, which is that of No. 22 in the above enumeration: but I have seen some French ones, of a different form, where the bob has been much thicker, and suspended by a cylindrical rod; and which would probably give a very different value of n, if subjected to actual experiment. The rods of the pendulums taken out by MM. Freycinet and Duperrey, were cylindrical and about ½ an inch in diameter: and it may be a matter of doubt whether the results with those pendulums are strictly comparable with the results obtained by pendulums of Captain Kater's construction. I have already shown, in the experiments with the pendulums No. 40 and 41, that a similar difference in the form of the rod only (the bob continuing the same) causes a difference in the result, amounting to upwards of 1·2 vibration in a day: and there may probably be other sources of discordancy which can be ascertained only by actual experiment. I fear therefore that, in deducing the true figure of the earth from pendulum experiments hitherto made, we can compare together only those experiments which are made with precisely the same kind of pendulums.

If we examine the new correction for the Mercurial clock pendulum, which is the pendulum now generally adopted for astronomical purposes, we shall find that a difference of one inch pressure of the atmosphere should produce an alteration, in the daily rate of the clock, equal to $0^s,414$; which is more than double the quantity hitherto assumed as depending on the change of the barometer; and which therefore can no longer be overlooked by the astronomer. In order to obviate this effect of a variation in the atmospheric pressure on the rate of the clock at the Observatory at Armagh, Dr. ROBINSON has recently attached a syphon barometer to the rod of the mercurial pendulum, so placed that the variations in the height of the column of mercury in the barometer may exactly compensate the effect produced by the change of atmospheric pressure. Mr. DAVIES GILBERT, in the Supplement to a paper inserted in the Quarterly Journal, vol. xv. has shown that the same compensating effect may be produced by a proper selection of the arc of vibration: since the effect produced by the difference of density in the atmosphere will, in such case, be exactly counterbalanced by the effect arising from the difference in the arc of vibration caused by such difference of density. And proceeding agreeably to the formula which he has there given for finding the value of such arc, and on the assumption of the accuracy of the new correction above mentioned, I find that the value of the required arc should be 2° 45′ on each side of the vertical line, or a total arc of 5½ degrees. I believe that the semi arc of vibration, in astronomical clocks, is seldom more than 2 degrees; which produces only one half of the compensating effect above alluded to: so that (assuming Mr. GILBERT's theory to be correct,) there still remains an effect on the daily rate of the mercurial clock, by a difference of one inch pressure of the atmosphere, of more than $\frac{2}{10}$ths of a second; which corresponds with the recent determinations of Dr. ROBINSON from observations made expressly for that purpose*. The attention of astronomers will probably in future be more particularly directed to this subject.

The values in the last column of the Table, denoting the weight of air adhering to the pendulum (supposed to be applied *to the centre of gyration*) follow a very different march from the values of the factor *n*; and lead to a more satisfactory explanation of the effect of the air on the motion of the pendulum. For, it evidently appears that the weight of air, *dragged* by a pendulum in motion, depends principally on the magnitude of the moving body; the influence of which however seems to be affected by other circumstances at present unknown: so that the exact law of the variation of this influence is not sufficiently apparent from the examples adduced: and further experiments are requisite to clear up this difficult but important point *.

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* See the Memoirs of the Royal Astronomical Society, vol. v. p. 125.

Difference in the two ends of a convertible pendulum.

If we examine the results of the several convertible pendulums given in the above Table, we shall find that the factor *n* is not the same for the two knife edges. This has been already noticed by M. Bessel, in his work so frequently alluded to, and also by M. Poisson in his recent paper in the Con. des Tems for 1834; both of whom seem to consider that the factor ought to be greater when the heaviest end is above the axis of suspension, than in the reversed position of the pendulum. This, however, does not appear to be universally the case, as will be seen by the following Table; where I have given the factors for the two knife edges of the several convertible pendulums used in the preceding experiments: together with the ratio between those factors, assuming as unity the factor for the knife edge A, or that position of the pendulum when the greatest weight is below the axis of suspension.

Factors for the correction of a convertible pendulum, for the reduction to a vacuum: with the ratio between the corrections for the two knife edges.

No.	Pendulums.	Knife edges.		Ratio.
		A.	B.	
23	Kater's, with wooden tail pieces	2·144	2·204	1·028
25	Brass bar, ¼ inch thick	1·847	1·968	1·066
27	Copper bar, ½ inch thick........................	1·891	1·991	1·053
29	Iron bar, ½ inch thick	1·945	2·064	1·061
31	Brass bar, ¾ inch thick	2·079	2·085	1·003
35	Brass tube, ½ inch diameter	2·318	2·262	0·976

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* The *Additional Experiments* which I have made on this subject, subsequent to the reading of this paper before the Society, will be given in page 438, &c.

From these comparisons it appears that although, in the cases of the first four pendulums, the correction for the knife edge B exceeds that of the knife edge A, yet in the case of the thick brass bar (No. 31), the corrections for the two knife edges are nearly equal; and in the case of the brass tube (No. 35), the correction for the knife edge B is smaller than that for the knife edge A; contrary to what takes place in the other pendulums, and contrary to the assumed theory on the subject *.

M. **Bessel** has suggested, as one of the modes of rendering the two knife edges, of a convertible pendulum, synchronous, to make the figure symmetrical, but the mass not so: which may be effected by making one part of the pendulum *hollow*. In such case, however, we must consider the hollow portion of the pendulum as a substance of a different specific gravity, and compute its effect on the vibrating mass accordingly. The results also, in such a case, will probably differ according as the hollow portion is hermetically sealed, or communicates freely with the circumambient air.

Additional Experiments.

Since this paper was read before the Society, I have made a number of *additional* experiments on other pendulums of different forms and construction, and have varied and combined some of the preceding pendulums in several new modes; with a view to clear up the anomalies apparent on the face of the preceding experiments, and to throw some light on the manner in which the air operates on the pendulum when in motion and affects the time of its vibrations. As the **Council** have given me permission to annex the substance of these experiments to the present paper, I shall briefly state the results obtained; together with such explanation relative thereto, as may be requisite for understanding the mode pursued, and the consequences deduced: but, I have not considered it necessary to encroach on this indulgence by giving the full detail of those experiments; which, however, it may be proper to state have been conducted on the same principles and with the same regard to accuracy as those already given in this paper. Indeed, it will be seen that there is less occasion for entering so minutely into the particulars of these experiments, since it will be found that the most material inferences deduced therefrom, do not depend on nice shades of difference in the results of the experiments, but that the cases are marked by broader lines of distinction; where the probable errors of observation and of computation would not make any appreciable difference in the results, or in the consequences to be deduced from them. Moreover, it will be seen that there is a regular march in the results of the several sets of experiments, which confirms the general accuracy of the whole: and it may be proper to state, once for all, that every value adduced is the result of at least *four* different experiments.

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* Probably the position of the two additional knife edges, with their knee-pieces, in the bar No. 31, and of the four additional planes, with their collars, in the tube No. 35, may have some influence in producing this apparent discordancy.

I believe it has generally been considered, by persons who have paid attention to this subject, that, in all funipendulous bodies in motion, the principal effect of the air, in adding to the inertia, is exerted on the *body* attached to the wire by which it is suspended; and that the *wire* itself (which is generally the finest that can be used with safety,) has little or no influence in producing any alteration in the time of vibration: and consequently all their experiments and investigations have been conducted under this view of the subject. This, probably, is not far from the truth in the most usual cases which occur, and have been considered: but, as it is desirable that the direct effect of the air on each portion of the pendulum should be separately and distinctly ascertained, as accurately as possible for all cases that are likely to occur, I instituted some new experiments with a view to determine this point.

In the pursuit of this inquiry I have found the suggestion and recommendation of Professor AIRY, "to ascertain the weight of air adhering to each pen-"dulum of experiment," of very essential service: as it has enabled me not only to mark the direct influence of the atmosphere on the pendulum much more accurately and distinctly than by merely deducing the value of the factor *n*: but likewise to distinguish its influence on the several parts of the pendulum. In many of the following experiments the march of the values, indicating such influence, appears at first sight very complicated and anomalous: for, in some of them, (see the 14th set,) the weight of adhesive air seems to be *less* when the spheres are attached than when the bare rod is used; and in others, (see the 19th set,) the weight of adhesive air dragged by a thin disc, appears to increase in a most extraordinary manner, merely by changing its position on the rod. But, I have been favoured by Professor Airy with the following investigation and remarks on this subject, which will clear up these and other seeming discordancies.

"It appears that the phenomena, to which you allude, may generally be explained by supposing a quantity of air, depending on the figure of the body, "to adhere to it whilst it is moving, and to add to its inertia without altering "its gravitation. In the experiments on bodies of a simple shape, the quantity of air is found, whose inertia, supposing it to adhere to the centre of "gyration, would account for the retardation of the pendulum (see page 431). "If then a compound body C consist of two parts A and B, (the distances of their "centres of gyration from the axis of motion being respectively c, a, b,) and if "the air adhering to the centres of gyration of A and B respectively were α "and β; then the compound pendulum C must be supposed loaded with the "inertia of α at the distance a, and of β at the distance b. The effect of these "would be the same as if the inertia of $\frac{\alpha a^2 + \beta b^2}{c^2}$ were applied at the distance "c. If then we find, as the result of experiment with the compound pendulum "C, that it has (from the action of the air) the inertia γ adhering to its centre "of gyration, we obtain the equation $\frac{\alpha a^2 + \beta b^2}{c^2} = \gamma$. Whence, the inertia due "to B alone, or

$$\beta = \gamma \left(\frac{c}{b}\right)^2 - \alpha \left(\frac{a}{b}\right)^2 \qquad (10)$$

"will be the correct measure of the adhesive air *dragged by that body alone.*"

We thus obtain a method of exhibiting separately the effect of the air on a sphere, cylinder, or other body (B) fastened to a rod (A) at any distance from the point of suspension. In the subsequent Tables therefore, I have annexed another collateral column, indicating in each case the effect of the air, or the increase of inertia, due to the suspended body alone, (without regard to the rod,) deduced agreeably to the above formula. I shall now proceed to the detail of the experiments; commencing with those which determine the effect due to the rods alone.

It will be seen that, amongst the preceding experiments, there are some made on a long brass cylindrical tube (No. 35—38), and on a long copper cylindrical rod (No. 21): and that the former, which is 1½ inch in diameter, gave a less value for the factor *n* than the latter which is little more than 4 tenths of an inch in diameter. Conceiving therefore that I might be enabled to determine the law by which such values were governed, I was induced to try other cylindrical rods, supported in the same manner as the copper one above mentioned, and of nearly the same length, but much smaller in diameter. I accordingly procured a brass rod, or wire, only 0·185 inch in diameter: in fact, it was a piece of the same kind of wire as that which was used with the solid brass cylinder No. 11, mentioned in the preceding part of this paper, page 410. I also caused one to be made about the same length, still smaller in diameter: but, as brass was not exactly suitable to such purpose, when so small, I procured one of steel, only 0·072 inch in diameter *. The length of the brass rod was 56·4 inches, it weighed 3106 grains and its specific gravity I found to be 8·444. The length of the steel rod was also 56·4 inches, its weight (including a small brass screw attached to the end) was 433 grains, and its specific gravity I found to be 7·687. Each of these, when in use, was screwed into the shank of the knife edge apparatus, which was 1·55 inch long, as already described in page 409. The results are contained in the following short Table: where I have continued the numbers of the pendulums from the preceding Table in page 433, for the sake of a convenient reference: No. 42 being No. 21 in the former list.

Thirteenth set.—*Results with plain cylindrical rods.*

Pendulum rods.	No.	*n*	Weight of adhesive air.
Copper, 58·8 inches long, 0·410 inch diameter	42	2·932	4·904
Brass, 56·4 inches long, 0·185 inch diameter	43	4·083 †	1·484
Steel, 56·4 inches long, 0·072 inch diameter	44	7·530	0·479

Now, here we find a regular increase in the value of *n*, as the diameter of the rod is diminished: and the inference is that, with a much smaller wire (such as is generally used in experiments with the pendulum,) the value of *n* would be considerably increased. But, to what limit this might extend I had no means of ascertaining, since the above steel wire was the finest that I could operate with: for, on account of its small weight, a pendulum of this kind soon comes to rest: and in order to guard against any probable error arising from this source, I took the mean of three consecutive sets of experiments, in determining each separate result. It also appears from these experiments that the quantity of adhesive air decreases as the diameter of the rod diminishes. For, a rod, about 59 inches long, and whose diameter is about 4 tenths of an inch, drags with it nearly 5 grains of air: whilst another rod of nearly the same length, and little more than one sixth of the diameter drags with it scarcely half a grain. But, although the thicker rod drags more air than the smaller one, yet the effect on the latter is much more considerable than on the former. For the 4·904 grains of air added to the weight of the copper rod, would reduce the specific gravity of the vibrating mass from 8·629 to 2·939 only: whilst the 0·479 grain of air added to the weight of the steel rod, would reduce the specific gravity of the vibrating mass from 7·687 to 1·024. And these are the respective specific gravities which if used in the computations for the reduction to a vacuum, would cause *n* to vanish *.

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* This was just 5 times the thickness of the iron wire, used in the preceding experiments, with the pendulums No. 1—20.

† I ought not to omit stating that this is the mean of eight different experiments, made with two different rods, at two different periods. Four of them (viz. two double ones,) were made with the wire here alluded to, on June 14th, and the others on August 2nd, with another piece of exactly the

Having thus ascertained the fact that the influence of the air is greater upon small rods than upon large ones (increasing considerably as the diameter of the rod diminishes), I next tried what effect would be produced by affixing various bodies to the ends of these rods. For this purpose I made use of the two brass spheres No. 3 and No. 6, already described in the preceding part

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same kind of wire, and having precisely the same specific gravity, but about half an inch longer. The results differ from each other more than I could have imagined; although each set is consistent in itself: for we have

	June 14.	Aug. 2.
	4·232	3·975
	4·179	3·947
Mean =	4·206	3·961
Weight of air =	1·536	1·431

I have examined all the steps of each experiment, and of the computations connected therewith; but cannot detect any source of error. In fact, it is one of those perplexing anomalies which occasionally occur in our researches after such minute quantities.

* I cannot trace the exact law of the variations in the three values in the column, indicating the weight of adhesive air dragged by each rod; but the nearest approximation is, that the numbers are nearly in the ratio of the square root of the cubes of their diameters.

of this paper; to which I added a third, 3 inches in diameter, weighing 29114 grains, and whose specific gravity I found to be 8·020. The ends of the brass and steel rods were screwed into the several spheres: but the copper rod was attached by means of an adapting screw. The results are given in the following Table: where it will be seen that in each of the three rods the value of *n* is diminished by appending either of the spheres thereto. The march of these values, however, does not appear to be very regular. Indeed, the conducting of the experiments when the spheres were attached to the *ends* of the rods, required great attention on account of the slowness of the vibrations, and the consequent frequency of the coincidences with the mean solar clock, with which they were compared; and they may consequently be subject to some little uncertainty *. In the case of the brass and steel rods the intervals of the coincidences did not exceed *eleven seconds*: but, on the other hand, I sometimes took a mean of several *thousand* of them, for the result.

Fourteenth set.—*Results with the spheres at the ends of the long rods.*

Diameter of the spheres.	Copper rod.				Brass rod.				Steel rod.			
	No.	*n*	Weight of adhesive air.	Weight due to sphere alone.	No.	*n*	Weight of adhesive air.	Weight due to sphere alone.	No.	*n*	Weight of adhesive air.	Weight due to sphere alone.
inches.												
0·00†	42	2·932	4·904	......	43	4·083	1·484	......	44	7·530	0·479	......
1·46	45	2·458	4·564	0·342	48	2·356	1·417	0·463	51	2·344	0·834	0·607‡
2·06	46	2·234	5·076	1·273	49	1·982	1·973	1·157	52	1·793	1·259	1·063
3·03	47	1·873	6·425	3·251	50	1·933	4·868	4·066‡	53	1·759	3·670	3·480

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* Should it be considered desirable to repeat these experiments with greater accuracy, arrangements might be made for that purpose, by altering the rate of the mean solar clock; which I was unwilling to disturb during the course of the present experiments.

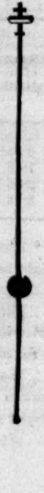
† The values in the first line are the same as those given in the preceding Table; and are here inserted in order to show their relative values as compared with the results when the spheres are attached to the rods. This plan will be pursued in the subsequent experiments.

‡ These two experiments (with the pendulums No. 50 and 51) are very unsatisfactory; and are marked as such in my journal. It was consequently my intention to have repeated them: but the subject was overlooked till it was too late. I should propose their being rejected altogether.

Now although there is enough on the face of the above experiments, to confirm the leading principles we are in search of, yet for the reasons already mentioned I should not select them as the most proper for the deduction of any very minute results, when compared with others made under more favourable circumstances.

If we examine the values, denoting the weight of adhesive air dragged by the compound pendulums, formed of the spheres attached to the ends of the several rods, they will be found to exhibit some apparent anomalies; more especially in the case of No. 45 and 48, where the weight of adhesive air seems to be *less* when the spheres are applied, than with the plain rod. But, it must be borne in mind that the deduced weight of adhesive air for each pendulum is in each case supposed to be applied to the *centre of gyration* (which is a different point of the rod, in each pendulum), and therefore requires correction. The collateral column, showing the weight due to the sphere alone (agreeably to the formula in page 440) will exhibit more accordance in the results; and denotes more distinctly the quantity we are in search of.

With a view of obtaining greater accuracy on the points in question, I resolved to try the effect of placing the spheres at, or near to, the centre of oscillation of the rods: whereby the above-mentioned inconvenient change in the intervals of the coincidences would be avoided, and the results rendered more trust-worthy. For this purpose I divided the brass and steel rods into two unequal parts at, or near to, the centre of oscillation: so that by screwing the longest of the two parts into the upper portion of the spheres, and the shortest into the lower portion, I might accomplish this object. But, as the whole length of the pendulum (from end to end) would, in such case, be longer than the rods, by the diameter of the inserted sphere, I cut off one inch from each part, in order that the length of the pendulum, from the knife edge to its extreme end, might, when thus used with the different spheres, be more nearly the length of the rods prior to the alteration. The two parts therefore of the rods, thus reduced, were 36·4 and 18·0 inches respectively. The copper rod was the property of Mr. TROUGHTON, and could not be thus divided. The following are the results with the spheres thus placed.



Fifteenth set.—*Results with the spheres at the centre of oscillation of the long rods.*

Diameter of the spheres.	Brass rod.				Steel rod.			
	No.	*n*	Weight of adhesive air.	Weight due to sphere alone.	No.	*n*	Weight of adhesive air.	Weight due to sphere alone.
inches. 0·00*	43	4·083	1·484	....	44	7·530	0·479	....
1·46	54	2·722	1·749	0·446	57	2·248	0·774	0·405
2·06	55	2·186	2·352	1·180	58	1·863	1·367	1·039
3·03	56	1·870	4·528	3·382	59	1·774	3·719	3·371

These experiments confirm the results of the preceding set, inasmuch as they show that, by fixing the spheres to this point of the rods also, the value of *n* is diminished: and there is moreover a greater regularity in the march of the values; as the intervals of the coincidences were much more adapted for correct observation. They consequently furnish us with the means of deducing with a greater probability of accuracy, the quantity of air adhering to, or dragged by, each of the spheres independent of the rod. These values are given in the preceding Table, and have been deduced agreeably to the formula to which I shall presently allude, on the assumption that the weight of air dragged by the brass and steel rods, is accurately shown in the 13th set of experiments. The following Table exhibits in a different form the values above alluded to.

Rods.	Diameter of the spheres.		
	1·46	2·06	3·03
Brass	0·446	1·180	3·382
Steel	0·405	1·039	3·371
Mean =	0·425	1·109	3·377

The quantity of air dragged by the two separate portions of a rod (whether it be actually divided, as in the present case, or a portion of its influence on the circumambient atmosphere be interrupted and destroyed, as in the case of the discs in the 18th and 19th sets of experiments,) as well as the distance of their centre of gyration from the axis of suspension, have been computed agreeably to the following formulæ, which have been obligingly furnished me by Professor Airy*.

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* See the first note in page 443.

“ Let r denote the weight of adhesive air dragged by one inch of the rod (equal, in the present cases, to $\frac{1}{56{\cdot}4}$ of the whole quantity dragged by these rods as found in the 13th set of experiments); and let us suppose that any one rod begins at x inches from the axis of motion, and ends at y inches from the same axis: then will the effect of the air adhering to that rod be represented by $\frac{r}{3}(y^3 - x^3)$. This is the same as if the whole quantity of air, $r(y-x)$, had been attached at the distance $\sqrt{\frac{y^3 - x^3}{3(y-x)}}$; which, in fact, is the distance of the centre of gyration of that rod from the axis of motion. The effect of the air adhering to several such rods will be represented by $\frac{r}{3}\Sigma(y^3 - x^3)$. Therefore the ratio which such quantity will bear to that carried by a rod of the length of the whole rod, if in one uninterrupted piece from end to end of the given pendulum, will be as $\frac{r}{3}\Sigma(y^3 - x^3)$ to $\frac{r}{3}(Y^3 - X^3)$; where X and Y are the distances, from the knife edge, of the extremities of the whole rod: whence, the weight of adhesive air, to be used in the formula (10), will be

$$\alpha = r(Y - X) \times \frac{\Sigma(y^3 - x^3)}{Y^3 - X^3} \qquad (11)$$

“ And the distance of the centre of gyration, from the axis of motion, for a system of rods, is

$$a = \sqrt{\frac{\Sigma(y^3 - x^3)}{3\,\Sigma(y - x)}} \qquad (12)$$

“ where, in each formula, $x = 0$ when the rod begins from the knife edge†.”

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[*] I am indebted to Professor Airy not only for these and other formulæ noticed in this paper, but also for various hints and suggestions during the progress of the experiments; and in general for the lively interest which he has taken in this inquiry: without which encouragement I certainly should not have extended the subject to its present length.

[†] It is in this manner that I have computed the weight of adhesive air due not only to the spheres in this set of experiments, but also to the cylinders and discs in the 17th, 18th, and 19th sets of experiments.

The values above given are nearly (although not exactly) in proportion to the cubes of the diameters: but, it is possible that some other element, at present unknown, may affect the results; and indeed some portion of the air may adhere to, or be dragged by the *sides* of the sphere. As the exact measure of these three brass spheres was, however, a matter of importance in this inquiry, I examined them more minutely, and found them to be 1·465, 2·065, and 3·030 inches respectively. So that the weight of adhesive air for the last two spheres will be almost exactly as the cubes of their diameters; and, for the first two, not materially differing therefrom. In fact, if the weights of air were ·387, 1·084, and 3·422 grains respectively, the whole would agree precisely with this hypothesis. It is worthy of remark that, in the case of the spheres No. 1 to 7, suspended by a wire (see the Table in page 433), and No. 66 in the following set, if we consider the weight of air, dragged by the wire alone, as equal to 0·10 grain, and deduct this value successively from the mean weight of air dragged by the 1·46 and the 2·06 inch spheres respectively, as there given, and by the 3·03 inch brass sphere as given in the following set of experiments, we shall have 0·373, 1·040, and 3·444 grains for the weight of air dragged by the spheres alone. So that, on the whole, I consider the hypothesis adduced as not far from the truth; and that the general expression for the quantity of air dragged by a pendulum consisting of a sphere suspended by a rod or wire, will be as follows: viz.

$$\mathrm{R} + \overset{\text{grains.}}{0{\cdot}123} \times d^3$$

where d denotes the diameter of the sphere in inches, and R the quantity of air dragged by the rod or wire. And if, in the case of a sphere suspended by a fine wire, of the length of the seconds pendulum, we suppose R to be (as already stated) equal to 0·10 grain, this formula will become

$$\cdot 002564\, l + \cdot 123\, d^3$$

where l denotes the length of the wire, in inches.

These values do not differ materially from those obtained by the same spheres attached to the *ends* of the *long* rods, as given in the 14th set of experiments: but I have already stated that those results were obtained under less favourable circumstances, and are not to be relied on with the same degree of confidence as the present set. They will be found however to accord more nearly with the following set of experiments where the spheres are attached to the *ends* of the *short* rods.

I next took away the lower rod from the spheres, and they were then attached to the upper rod only; whereby the pendulums became nearly of the same length as No. 3 and No. 6, mentioned in the preceding part of this paper: with the results of which it was my object to compare them. And as the 3 inch brass sphere had not yet been swung with the iron wire, I now made some experiments with this mode of suspension, for the express purpose of the comparison[*]. The following are the results:

Sixteenth set.—*Results with spheres at the end of the short rods.*

Diameter of the spheres.	Brass rod.				Steel rod.				Iron wire.			
	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.	No.	n	Weight of adhesive air.	Weight due to sphere alone.
inches.												
1·46	60	2·198	1·047	0·465	63	1·904	0·537	0·410	3	1·834	0·457	0·357
2·06	61	1·901	1·513	1·078	64	1·785	1·227	1·104	6	1·751	1·140	1·040
3·03	62	1·830	4·202	3·719	65	1·779	3·720	3·587	66	1·748	3·544	3·444

If the results with these brass and steel short rods be compared with those of the same spheres attached to the end of the long rods, stated in page 443, we shall find that as far as the value of n is concerned, it is, with one slight exception, greater in long pendulums than in short ones: but, the difference appears to depend chiefly on the relative magnitudes of the spheres and of the rods. With respect to the weight of adhesive air I regret that I could not conveniently swing these short rods without the spheres attached thereto; which would have enabled me to ascertain (agreeably to the formula in page 440), whether the weight of air adhering to, or *dragged* by, each sphere respectively is the same in this set of experiments, as in the preceding set. But, if we suppose that the weight of air, dragged by these short rods, is proportional to their lengths, and employ such quantities in the formula above mentioned, we shall find that the weight due to the spheres alone, when attached to the brass and steel rods, will be as stated in the preceding Table. The values annexed to the spheres, when suspended by the iron wire, are deduced from the assumption that the weight of the air dragged by the wire is equal to 0·10 grain, as already stated. These values, like most of those deduced from the 14th set of experiments, agree very well with those which result from the spheres when annexed at the centre of oscillation: and the whole show that the effect of the air on a pendulum consisting of a sphere suspended by a fine rod or wire, although principally due to the sphere, is partly owing to the wire also: but that this influence of the wire diminishes with its diameter; and, when extremely fine, probably becomes a small constant quantity, of nearly equal value in the most usual cases that occur*.

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[*] The iron wire used with this heavy sphere was ·023 inch in diameter; or about one third of the thickness of the steel rod; and nearly twice the thickness of the wire used in the experiments with the pendulums No. 1 to No. 20. It weighed 26 grains.

In order to place the subject of this inquiry in a clearer point of view with respect to other bodies, I caused three additional brass cylinders to be made; which, with the cylinder No. 10, described in the preceding part of this paper, were proposed to form the subject of a new set of experiments. The diameters of all these cylinders were ordered to be made exactly alike; viz. 2·06 inches: and their respective heights, or thicknesses, were 2·06 inches, 1·00 inch, 0·50 inch, and 0·18 inch. This latter thickness was chosen on account of its being precisely the diameter of the brass rod. The 1 inch cylinder weighed 6611 grains, and its specific gravity I found to be 7·805: the ½ inch cylinder weighed 3352 grains, and its specific gravity I found to be 8·116: and the ·18 inch cylinder weighed 1266½ grains, and its specific gravity I found to be 8·145. The other cylinder has been already described. All these cylinders were tapped in the circumference, with two screw holes, opposite to each other, for the purpose of affixing thereto the two unequal portions of the rods above mentioned: whereby the cylinders became placed nearly in the centre of oscillation of the whole length of the rod. The cylinders, thus placed, were swung with their flat sides vertical, and opposed to the line of motion; similar to the pendulum No. 12, as described in the preceding part of this paper. The following are the results.

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* This appears from the slight difference in the quantity of adhesive air dragged by the steel rod and iron wire, in this set of experiments; which is very small. And moreover, in the case of the ivory sphere (No. 4), which was suspended by a *very fine* silver wire, the result is precisely the mean of the other spheres, which were suspended by the *much coarser* iron wire.

Seventeenth set.—*Results with the 2-inch cylinders placed at the centre of oscillation of the long rods.*

Thickness of the cylinders.	Brass rod.				Steel rod.			
	No.	*n*	Weight of adhesive air.	Weight due to cylinder alone.	No.	*n*	Weight of adhesive air.	Weight due to cylinder alone.
inches. 0·00*	43	4·083	1·484	......	44	7·530	0·479	......
0·18	67	5·547	2·852	1·284	71	7·694	1·806	1·350
0·50	68	3·941	2·942	1·523	72	4·136	1·900	1·490
1·00	69	2·892	2·972	1·681	73	2·745	2·046	1·661
2·06	70	2·141	3·111	1·902	74	1·988	2·309	1·946

Here we find a regular increase in the value of *n*, as the thickness of the cylinder diminishes; till it approaches nearly equal to the thickness of the rod itself, when the effect of the cylinder on the value of *n* is eliminated, and the result is the same as if no cylinder were attached to the rod. Passing this point, and the thickness of the cylinder becoming equal to, or less than, the diameter of the rod, the effect of the cylinder becomes positive; and the value of *n* is now greater than when the rods are swung without any thing attached thereto. Setting aside however the value of *n*, and confining our attention to the quantity of air adhering to, or dragged by, these pendulums, we find that it varies with the thickness of the cylinders. And, pursuing the same steps, as in the case of the spheres (see page 446), we obtain the values above given, for the weight of air due to the cylinders alone; and which are more conveniently arranged in the following form: viz.

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* See the first note in page 443. It must be noted here that in the first horizontal line, no cylinder is supposed to be attached to the rod: and therefore these values are not directly comparable with the rest.

Rods.	Thickness of the 2-inch cylinders.			
	0·18	0·50	1·00	2·06
Brass	1·284	1·523	1·681	1·902
Steel	1·350	1·490	1·661	1·946
Mean =	1·317	1·506	1·671	1·924

The *differences* between these mean values would indicate the quantity of air dragged by the *sides* of a cylinder of this diameter, according to its thickness: but which does not appear to be very regular in its march; since the thin cylinders drag more in proportion than the thicker ones. Till this fact is more fully ascertained, we cannot deduce a correct general formula for determining the quantity of air dragged by cylinders of different diameters and thicknesses, swung in the manner above mentioned.

The next set of experiments were made with thin circular discs of brass, having about the same thickness as common thick post paper. Twenty pieces, screwed together in a vice, measured ·08 inch; consequently the thickness of each of the brass discs may be assumed equal to ·004 inch. One of these discs was intended to be 2·06 in diameter, in order to correspond with the cylinders above mentioned; but it is in fact somewhat larger, being 2·07; and weighs 28 grains: the second was 3·01 inches in diameter, and weighed 57·5 grains: and the third was 4 inches in diameter, and weighed 106·5 grains. Their specific gravity I found to be 8·450. The long brass rod above mentioned* was then tapped with a screw hole at 38 inches from the knife edge, and the three discs, in succession, were respectively fastened thereto; and swung with their flat sides opposed to the line of motion. The long steel rod could not be used on this occasion, not only because the discs could not be conveniently attached thereto, but also on account of its coming to rest so soon.

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* This was the *second* brass rod, 56·9 inches long, mentioned in the second note in page 441.

Eighteenth set.—*Results with the thin brass discs placed near the centre of oscillation of the long brass rod**.*

Diameter of the disc.	No.	n	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·00†	43	4·083	1·484	....
2·07	75	7·439	3·111	1·405
3·01	76	14·362	6·511	4·185
4·00	77	27·033	12·873	9·367

In these experiments the value of n, and also the weight of air *dragged* by the pendulum increase as the diameter of the disc increases. If we examine the values in the last column (in computing which, the quantity of air dragged by the rod has been assumed of a different value in each case, or proportionate to the length of the rod, *minus* the diameter of the disc), we shall find that the quantity of air dragged by these thin discs, is also nearly in the ratio of the cubes of their diameters: and the general expression for the amount of the same will be nearly

$$\text{R} + \overset{\text{grains.}}{0{\cdot}149}\, d^3$$

R and d denoting the same quantities as before.

With a view of following up this inquiry relative to the discs, I caused the same brass rod to be tapped with 3 other screw holes: one at 5·1 inches from the knife edge, being the highest point to which I could fix anything; another at 30·0 inches from the knife edge, or near the centre of gravity of the rod; and the other at 57·3 inches from the knife edge, or near the lower end of the rod. The thin brass disc, 2·07 inches in diameter, was then successively fastened to the rod, at each of these distances, and swung in the same manner as in the preceding set, with the flat sides opposed to the line of motion. The following are the results; including that of No. 75 given in the preceding set.

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* Owing to some mistake all these discs were placed 8 tenths of an inch *above* the centre of oscillation. This is allowed for in the computations for the weight due to the disc alone.

† See the first note in page 443.

Nineteenth set.—*Results with the 2-inch thin brass disc, placed at different distances from the knife edge on the long brass rod.*

Distance from knife edge.	No.	*n*	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·0*	43	4·083	1·484	....
5·1	78	4·155	1·523	....
30·0	79	6·115	2·457	1·330
38·0	75	7·439	3·111	1·405
57·3	80	12·124	5·368	1·426

The differences in the weight of adhesive air appear, at first sight, very anomalous: especially when we consider that the vibrating specific gravity of the mass, and the weight of the pendulum, are *exactly alike* in each case. But, it should be remembered that these weights of adhesive air are supposed, by the formula in page 431, to be applied to the *centre of gyration*: and, if we wish to determine the effect due to the disc alone, we must have recourse to the formula in page 440. It is in this manner that I have obtained the results given in the last column of the preceding Table, under the head of "Weight due to the disc alone." The mean of the last three values gives the weight of air due to the disc alone, equal to 1·387 grain. I have not included the case where the disc was only 5·1 inches from the knife edge; since no dependence can be placed on the result, on account of the magnitude of the multiplier. In fact, if the weight of adhesive air at that point of the rod, were only 1·466 instead of 1·523, the weight due to the disc alone would correspond with the mean of the rest.

As I was desirous of varying these experiments as much as possible, I next tried the effect of two of the thinnest of the cylinders above mentioned (having the same diameter as the disc used in the preceding set of experiments), whose thickness was respectively 0·18, and 0·50 inch: in order to see whether they would exhibit the same law. The cylinders were screwed to the *end* of the long brass rod; and swung, as in the preceding set, with their flat sides opposed to the line of motion. The following are the results:

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* See the first note in page 443.

Twentieth set.—*Results with the 2-inch cylinders placed at the end of the long brass rod.*

Thickness of the cylinders.	No.	n	Weight of adhesive air.	Weight due to cylinder alone.
inch.				
0·00*	43	4·083	1·484	....
0·18	81	6·216	3·590	1·389
0·50	82	4·046	3·117	1·628

These results are somewhat greater than those deduced from experiments with the same cylinders in the 17th set; but here I should repeat the remark already alluded to in page 444. In fact, on referring to the observation book, I find that the intervals of the coincidences were only 14 seconds: and I fear that a sufficient number of them were not taken, to insure that degree of accuracy which is requisite in such minute inquiries. I should therefore give the preference to the preceding set of experiments.

The next and last class of experiments was necessarily very limited; as, from the construction of my vacuum apparatus, I could not conveniently extend them so far as I could wish. They were instituted for the purpose of determining the difference between the results of the brass cylinders, and the thin brass discs swung *edgeways*, and the results when swung in the manner already described; namely, with their *flat sides* opposed to the line of motion. The two cylinders, used in the preceding set, and the two discs respectively 2·07 and 3·01 inches in diameter, were chosen for this purpose. The two cylinders were screwed, as before, to the *end* of the long brass rod; in order to compare their results with the preceding set: but the two discs were screwed to the brass rod, at 38 inches from the knife edge, in order to compare their results with those of No. 75 in the eighteenth set. The several results are as follow:

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* See the first note in page 443.

Twenty-first set.—*Results with the 2-inch cylinders, placed edgeways, at the end of the long brass rod.*

Thickness of the cylinder.	No.	*n*	Weight of adhesive air.	Weight due to cylinder alone.
inch.				
0·00*	43	4·083	1·484	....
0·18	83	2·771	1·219	0·149
0·50	84	2·208	1·239	0·353

Twenty-second set.—*Results with the thin brass discs, placed edgeways, near the centre of oscillation of the long brass rod.*

Diameter of the disc.	No.	*n*	Weight of adhesive air.	Weight due to disc alone.
inches.				
0·00*	43	4·083	1·484	....
2·07	85	4·291	1·588	0·091
3·01	86	4·472	1·675	0·168

These experiments confirm the remark already made, that the *sides* of the moving body drag with them very little of the air which has so remarkable an effect on the pendulum. In this last set, the discs were placed (as in the 18th set), at 8 tenths of an inch above the centre of oscillation. In making the computations for the weight due to the disc alone, this has been allowed for: and I would also observe that the *whole* effect of the rod has been used in those computations; as it is evident that the position of the disc does not obstruct any part of its action on the air.

General results of the Additional Experiments.

I must here close the account of these *additional experiments*, which have been pursued up to the latest moment that could be conveniently spared by the printer; as I was desirous of communicating at once all the information I could procure on this interesting subject: and which consequently leaves me only time to offer a few brief remarks on the results obtained.

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* See the first note in page 443.

It appears then that all these results accord with the theory that a quantity of air adheres to every pendulum when in motion: and, by thus forming a portion of the moving body, diminishes its specific gravity; or, rather adds to its inertia. This adhesive air is confined almost wholly to the two opposite portions of the pendulum, which lie in the line of its motion; (similar to what takes place with a body moving through still water), and very little of it adheres to, or is dragged by, the *sides* of the pendulum. The shape of this coating of air will consequently partake, in some measure, of the form of the pendulum; subject probably to some slight modifications, with the nature of which, however, we are at present unacquainted. The quantity of air dragged by a pendulum seems to depend on the extent and form of surface opposed to its action, and is not affected by the density of the body.

In the case of a *sphere*, 1 inch in diameter, suspended by a fine wire, the weight of air dragged by the sphere alone appears to be about 0·123 grain troy: and for spheres of any other diameter, in nearly the direct ratio of the cubes of their diameters. The weight of air dragged by the wire (of the length of the seconds pendulum), may amount to 0·10 grain, but probably does not exceed that quantity; and perhaps is nearly constant for all *fine* wires of that length: so that with small spheres (less than 1 inch in diameter), the weight of air dragged by the *wire*, is nearly the same as that dragged by the *sphere*.

With respect to *cylinders* suspended by rods, and swung with their flat sides opposed to the line of motion, the law of the variation is not so manifest; as we are at present ignorant of the precise effect caused by the *edge* of the cylinder. Neither have we obtained sufficient data to develope the effect of the air on cylinders, suspended by rods or wires, and swung with their flat sides in a horizontal position; similar to the pendulums No. 10 and 14. In these cases (see page 433), the 4 inch cylinder drags *much more* than double the quantity of air adhering to the 2 inch cylinder: although they have precisely the same diameter. And these are the only experiments, which I have made, connected with this branch of the subject.

With respect to very thin cylinders, or *discs*, swung with their flat sides opposed to the line of motion, the weight of air dragged by a disc, of 1 inch in diameter, appears to be about 0·149 grain; and for discs of any other diameter, nearly in the direct ratio of the cubes of their diameters. Whence it appears that a thin *disc* drags *more* air than a *sphere* of the same diameter.

As the quantity of air dragged by spheres is proportionate to the cubes of their diameters, I was induced to examine whether the quantity dragged by a sphere, and by a cylinder of the same diameter and height, would be proportionate to their solid contents; or, in the ratio of 1 to 1½. But, from a comparison of pendulums No. 6 and 10 (see page 433) it appears that the cylinder drags more than that proportion, by about ⅛th part of the whole.

If we compare the results of pendulums No. 10 and 13 (see page 433), the difference in the quantity of air dragged would appear to be that which is due to the difference in the effect produced by the wire and the rod. But we must bear in mind what has been stated in page 440, relative to bodies suspended at the *end* of a rod or wire; and reduce them, by the formula there given, to the same point: in which case, the weight of adhesive air, *due to the cylinder alone*, would be very nearly alike in both experiments.

From a review of the whole, it appears that even when a pendulum is formed of materials having the same specific gravity, yet, if it be not of an uniform *shape* throughout, each distinct portion must be made the subject of a separate computation, in order to determine the correct *vibrating* specific gravity of the body; since each part will be variously affected by the circumambient air. As an example, take the case of the pendulum No. 3, where the iron wire and the brass sphere have almost exactly the same specific gravity, viz. 7·66. If we suppose the sphere drags 0·40 grain of air, and the wire 0·10 grain, (or about ¼ of that dragged by the sphere), we shall have the specific gravity of the sphere, with its coating of air, reduced to about 4·43, and that of the wire with its coating of air, to about 0·14. Whence the *vibrating* specific gravity of the whole pendulum, deduced agreeably to the formula (2) in page 405, will be about 4·21; which would give the reduction to a vacuum equal to 13·380 seconds: differing very little from the true correction given in the Table in page 433. If the effect of the air on the wire had been neglected, this value would have been diminished about one second: which shows that in making experiments on pendulums of this kind in *water*, the *whole of the wire* should be immersed in the fluid, in order to deduce correct results.

In concluding these experiments I cannot flatter myself that no error has escaped me; especially when I consider the vast number of computations which have been employed in these investigations. The major part of them, however, have been revised, especially those which exhibited any remarkable anomaly: and I trust that no error of importance will be found to exist. During the progress of the experiments, the apparatus has been from time to time altered, in order to suit the circumstances of the case: and trifling differences of specific gravity, and of comparative lengths and weights arising therefrom, may consequently have passed unnoticed and unobserved. Indeed the subject has been altogether so new, that in commencing a set of experiments, I was not always aware of the precise points, to which it was most necessary to direct the attention; and which were not sufficiently apparent till after the result was obtained. Should it however be desirable to repeat any of these experiments, in a manner that may be considered likely to lead to more accurate results, I shall be happy to resume the enquiry.

The Chevalier Du Buat's *Experiments.*

During the course of these enquiries, it will be seen that I have, all along, considered M. Bessel as the first discoverer of that peculiar property of the *moving* pendulum, which it has been the object of this paper to elucidate: and undoubtedly, he is entitled to the merit of having *first* applied those principles, which he has investigated with so much accuracy and with such great ability, to the *modern pendulum*; and thus rendered it a more powerful and delicate instrument in the hands of the practical and theoretical philosopher. But, it has recently been found that this same property of the pendulum was known nearly fifty years ago, and distinctly treated by the Chevalier Du Buat in his Principes d'Hydraulique. In that work, the second edition of which appeared in 1786*, the author has stated the results of a number of experiments on pendulums of various kinds, swung in air and in water; from which he was led to infer that a quantity of the fluid in which the pendulum oscillates, is *dragged* with it in its motion, and thus retards its vibrations. He remarks that "if a "denotes the length of a pendulum making any number of vibrations in vacuo, "l the length of a pendulum making the same number of vibrations in the "fluid, p the weight of the moving body in the fluid, P the weight of the fluid

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* The *first* edition (1779) does not contain the experiments here alluded to.

"displaced by the body; then $p + \mathrm{P}$ will express its weight in vacuo, and "$\frac{p+\mathrm{P}}{p}$ will be the ratio of gravity in the two cases: whence we obtain

$$l = a \times \frac{p}{p+\mathrm{P}}$$

"This formula would give correctly the length of the pendulum, if the body "in moving did not *drag* with it a certain quantity of the same fluid, which "varies very little by the difference of velocity: so that the mass, when in "motion, consists not only of the mass of the body itself, but also of the fluid "*dragged with it* *." He then proceeds to show (page 229) that "if n be any "constant number such that $n\,\mathrm{P}$ expresses in all cases the weight of the fluid "displaced and *also that of the dragged fluid*, the mass, when in motion (or its "weight in vacuo) is no longer $p + \mathrm{P}$, but is represented by $p + n\,\mathrm{P}$; whilst "its weight in water is always expressed by p. The correct formula therefore "will be

$$l = a \times \frac{p}{p+n\,\mathrm{P}}$$

"whence we deduce

$$n = \frac{p}{\mathrm{P}}\left(\frac{a}{l} - 1\right)"$$

M. Du Buat then gives the result of 44 experiments made by swinging pendulums formed of spheres of lead, glass and wood, of different weights, and suspended by lines of different lengths: and the conclusion at which he arrives is, that the value of n (which, in his experiments, varies, with only 4 slight exceptions, from 1·67 to 1·45) may be assumed equal to 1·585 †. This certainly agrees with the fact much more nearly than might be expected from the rough manner in which those enquiries were conducted, as compared with more modern experiments. And, although it cannot be placed in competition with the more rigid investigations of M. Bessel, or the results detailed in this paper, yet it evinces the great talent and zeal of the author in being able to extract so near an approximation from such a mode of procedure. M. Du Buat then gives the result also of a vast variety of similar experiments on cylinders, prisms, cubes, &c.: and found in each of them a complete confirmation of his opinion relative to the *dragging* of the fluid in which the vibrations were made. And although he remarks that the above mean value of *n* is given as generally suitable to all cases of spheres, yet he suspects that the quantity of *dragged fluid* is rather less with large spheres than with small ones, and also that it is rather less for short pendulums than for long ones *.

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* Edition 1816, vol. ii. page 226.

† Ibid. page 257.

But, is it not a remarkable circumstance in the history of this subject, that these important and apparently conclusive experiments of M. **Du Buat**, which were made by the order and at the expense of the French Government, which were examined, at the request of the Minister of War, by the Royal Academy of Sciences at Paris, and by them favourably reported on, which were first published in the year 1786 (little more than 10 years prior to the experiments of M. **Borda** on the length of the pendulum †), and which excited so much interest that they led to the subject for the Prize Essay, proposed by the Academy in the following year; and which, not being answered, was repeated in the year 1791, with the offer of a *double* reward;—experiments which attracted at that time so much public attention that another edition of the work appeared in 1816, just about the time when the subject of the pendulum was revived in the different states of Europe; which has not only been translated into the German language ‡, and praised in the highest terms by some of their principal writers on that subject, but has been also largely quoted in many English works, and freely commented on in this country:—is it not singular that such experiments should have been so soon and so completely lost sight of, and forgotten, that not one of the many distinguished individuals actually engaged in those pursuits, and in the investigation of this subject, should have had the least idea or remembrance of the additional correction for the reduction to a vacuum so clearly pointed out by M. **Du Buat**: and that until the re-discovery of this principle by M. **Bessel**, as detailed in his valuable paper on the pendulum, no one should have thought of verifying the suspicion of **Newton** that such an effect was probable [*]. M. **Prony**, in his Nouvelle Architecture Hydraulique, and Dr. **Young**, in his Lectures on Natural Philosophy (both of whom have taken an active part in the investigations relative to the pendulum) make frequent allusions to **Du Buat's** work: yet neither of these distinguished mathematicians appears to have recollected the singular facts recorded by that author. And even in M. **Poisson's** late excellent memoir, inserted in the Connaissance des Tems for 1834, although in the Appendix thereto the author's attention has been called to M. **Du Buat's** experiments by a notice from another quarter, yet it is evident that when that distinguished mathematician commenced his paper, he was not aware of the facts stated in M. **Du Buat's** work: as he frequently, and very justly, alludes to M. **Bessel** as the *first* person who had directed the attention of the public to the true correction. And it certainly is but a poor consolation to the practical philosopher, who thus devotes so much of his time to the elucidation of any particular branch of science, to find that his labours may be *so soon* forgotten, and probably lost sight of for ever.

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* The first suspicion is verified by the present experiments; at least, in the light in which M. **Du Buat** viewed the subject. For though the quantity of dragged fluid is greater with large spheres than with small ones, yet the factor *n*, which he appears to have considered its index, is less. The second suspicion is also confirmed not only by some of the present experiments, but likewise by those of M. **Bessel**, alluded to in the note in page 434.

† I am unable to fix the precise date of M. **Borda**'s experiments: for, although the month and the day, as well as the exact time to the nearest *second*, are minutely recorded, I have not been able to detect the *year* in which they were made.

‡ By J. F. **Lempe**, Leipsic 1796. See also the works of **Langsdorf**, and others.

Suspension over a Cylinder.

The principal portion of M. **Bessel's** experiments on the pendulum were made by suspending the sphere, by means of a wire, over a steel cylinder not more than ·088 of an inch in diameter. Being desirous of pursuing the same plan with respect to some of the pendulums which are the subject of this paper, I suspended the lead and ivory spheres (No. 8 and 9) in this manner; the results of which have been already stated. I proceeded in a similar manner with some of the other pendulums; but in the course of the experiments I discovered some anomalies, for which I could not at first satisfactorily account; and at length found that they proceeded altogether from the mode of suspension. In the long cylindrical rod (No. 21) the discordancies were the most apparent: for not only would the intervals of consecutive coincidences differ from one another as much as 60, 70 and in one case as much as 90 seconds (*plus* and *minus*), but the arc also would be continually varying in magnitude in a similar manner, alternately diminishing and *increasing*. With a view to discover the cause of these singular anomalies, I erected an apparatus for more minutely observing and watching the motion of the pendulum during its vibrations: and I found that when the sphere was suspended by a wire over a cylinder, the motion of the ball, although set off in a straight line, soon became elliptical; that the eccentricity of the ellipse was continually diminishing; and that the major axis was continually shifting its position with respect to the points of the compass: circumstances which were sufficient to account for all the appearances above described, and to destroy all confidence in experiments conducted in such a manner. And although I have retained the experiments with the pendulums No. 8 and 9, above alluded to, which were made in this way; yet it has been more to show the near accordance which may sometimes be accidentally attained by an incorrect method, and that we cannot examine too minutely into every step of so delicate an inquiry.

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[*] Principia, lib. ii. prop. 27. cor. 2.

I wish it however to be fully understood that these remarks do not apply to M. Bessel's experiments, since there is this important distinction to be made between his mode of proceeding and mine: viz. that his wire, at the part where it passed over the cylinder, was purposely made *flat*, probably with a view of avoiding this very difficulty; whereas mine was *round*, as generally sold in the shops. I have not yet tried the *flat* wire, but have thought it right to point out the inaccuracies that may attend the use of the *round* wire, in order that others may not adopt it without the precaution of first ascertaining how far the results of any experiments may be affected by the anomalies above alluded to. In conclusion, I would add that, in the knife edge suspension, the vibrations of the ball were uniformly preserved in a straight line during the whole time it was in motion: and no anomalies were discoverable.

Confined space of the Vacuum apparatus.

It has been suggested by some persons that the results of experiments, of the kind mentioned in this paper, may probably be affected by the confined space of the tube in which the oscillations of the pendulum are made. M. Poisson, in his valuable memoir above alluded to, has justly stated that, in all the analytical investigations, the oscillations are supposed to be made in a fluid which extends indefinitely in all directions: a circumstance, however, which cannot practically take place in experiments of this kind. But he imagines that when the pendulum is small, in comparison with the dimensions of the inclosed space, the results are not sensibly affected: and that they are least so, when the surface of the confining body is curved. In the Greenwich vacuum apparatus, where the tube is about 13 inches in diameter, Captain **Sabine** did not find any difference in the results of some experiments instituted for the express purpose of ascertaining the same; although the bob of his pendulum was 6 inches in diameter. In my own apparatus also, I have found the results of numerous experiments with the *bar* pendulums within the tube, agree very well with those in free air, before the vacuum apparatus was erected: and certainly no discordance has been observable, sufficient to warrant any material alteration in the results of the present experiments. In the Greenwich apparatus, the glass cylinder is formed of three separate pieces, which may be easily taken apart; and the pendulum may thus be, at any time, exposed to the free air: whereby the experiments may be alternately made in the confined cylinder, and in the free air. But my apparatus consists of one uniform brass tube, and is not adapted to such a change of experiments.

Anomalies of the knife edge suspension.

It has been shown by Captain **Sabine**, in his Account of Experiments, &c. page 195, that, in a pendulum with knife edges, a considerable difference may arise in the results, if they be used with *different planes*: but it does not appear to have occurred to any one, versed in these experiments, that a much greater difference than that which he has recorded may arise from using the *same* knife edge with the *same* plane. This fact has probably hitherto escaped detection from the peculiar manner in which pendulum experiments are usually conducted: for, on examining the detail of most of those experiments, it will be found that after the pendulum at any one station has been placed in its Y's, it has never been removed therefrom, but merely raised and lowered again as occasion may require, till it has been ultimately dismounted, and packed up for another station; whereby any anomaly that might otherwise have occurred, is thus avoided, and consequently escapes detection. Experiments, however, of this kind, ought to be varied in every possible way, in order to guard against any unsuspected source of error.

When Captain **Basil Hall** returned from his voyage to the Pacific Ocean, where he had undertaken to swing the pendulum at various places, it was found that the number of vibrations which the pendulum made, on his arrival again in London, differed by 0·97 (or not quite a second of time) in a mean solar day, from the number of vibrations made by the same pendulum previous to his departure: and various causes were assigned for (what was called) so great and so singular a discordance[*]: for, I believe, at that time the results with the invariable pendulum were considered almost as infallible. It is true that we have a few instances of a contrary nature, where the pendulums, on their return home, have told precisely the same story as they did when they were sent off; and, in the case of the two pendulums that were taken out by Captain SABINE, *their coincidence during the whole of the voyage was very remarkable*, since the greatest variation from the mean did not exceed 0·32 at any one of the stations[†]; but, these I consider rather as singularly favourable circumstances in his particular case, than as tending to invalidate the results of other experiments. In the voyage of Captain FREYCINET, who took out three separate pendulums, we find a variation in the difference between them, amounting to several seconds in a day. Thus, at the Isle of Guam the difference between pendulums No. 1 and No. 2 was 1180·162 vibrations; whereas at the Isle of Rawâk the difference was only 1173·693; being a variation of 6·469 vibrations. At the Isle of France the difference between pendulums No. 2 and No. 3 was 1012·326 vibrations; whereas at the Isle of Rawâk, the difference was only 1008·557; being a variation of 3·769 vibrations. And at the Isle of France the difference between pendulums No. 1 and No. 3 was 164·948 vibrations; whereas at the Isle of Guam the difference amounted to 169·833; being a variation of 4·885 vibrations in a mean solar day[‡]. Captain DUPERREY also, who took out two of these same pendulums (No. 1 and No. 3) in a subsequent voyage found the difference between them, at the Malouine Islands, to be 169·931 vibrations; whereas on his return to Paris the difference was only 168·235; being a variation of 1·696 vibration[§]. Now, in all these cases there ought to be little or no variation in the difference between any two of the pendulums; neither would there be if we could insure the making of the experiments precisely under the same circumstances; and no blame can be attached to those zealous officers, surrounded as they must be with difficulties of every kind for carrying on such delicate experiments. In fact, amongst the multitude of experiments that I have myself made, I have seldom found, after I had *dismounted* a pendulum, and then replaced it (even on the same day, under all the favourable circumstances of equality of temperature &c., and with all the conveniencies of manipulation,) that I could make it tell the same story in the next series of experiments. Even the same pendulum, furnished with two different knife edges, rendered synchronous or nearly so, similar to those described in the above enumeration as convertible pendulums (No. 25—38), where the trifling difference in the results of each pair of knife edges, ought, after proper reductions, to be a constant quantity, will frequently differ by an amount much greater than can be attributed to the errors of observation.

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[*] Phil. Trans. for 1823, page 287.

[†] Account of Experiments, &c. page 189.

[‡] Voyage autour du Monde, par M. FREYCINET, (Observations du Pendule,) page 22.

[§] Connaissance des Tems for 1826, pages 294 and 300.

The fact, I believe to be, that the pendulum furnished with a knife edge and agate planes, as at present constructed, is a very inadequate instrument for the delicate purposes for which it was originally intended: and a more rigid examination and adjustment of that part of the instrument are requisite, before we can depend on the experiments made with it, either for the determination of the length of the seconds pendulum, or even for the comparison of results obtained in different parts of the world. The knife edge is seldom or never perfectly straight; the planes are seldom or never perfectly true: at least, I have never found one so, amongst the number of those on which I have experimented. The consequence is that, as there is generally a little play in the Y's, the knife edge is not always let down on the same parts of the agate plane. This may be best detected by holding a lighted candle behind the knife edge when it is resting on the plane: by which method the smallest inequalities in the points of contact are readily discernible. But, the fact is rendered still more evident by reversing the pendulum in the Y's, when a sensible difference in the result generally takes place. Amongst the numerous pendulums in my possession, I have not met with more than one, that does not differ in the results by an appreciable quantity, when the pendulum is reversed in the Y's, or turned half round in azimuth. If the knife edge and planes were perfectly correct and true, there ought not to be any difference in the results, whichever side of the pendulum is placed fronting the observer: how then are we to account for a difference of upwards of two vibrations in a day which actually occurs in one of the pendulums above alluded to! The following Table, however, will set this matter in a clearer point of view, and show the real differences which I have found to take place in the results, merely by reversing the face of the pendulum. The numbers, in the first column, have reference to the enumeration of the pendulums in the preceding part of this paper. The brass bar, $\frac{3}{8}$ of an inch thick, was swung on two different agate planes; and the results by no means accord with each other.

Differences in the results, by merely turning the face of the pendulum.

No.	Pendulums.		Difference.
1—21	French knife edge		0·249
22	Kater's invariable, No. 11		0·914
25	Brass bar, $\frac{3}{8}$ inch thick	knife edge A	0·135
26		knife edge B	0·939
25	Same Brass bar on other planes	knife edge A	0·725
26		knife edge B	1·078
27	Copper bar $\frac{1}{2}$ inch thick	knife edge A	0·296
28		knife edge B	0·171
29	Iron bar $\frac{1}{2}$ inch thick	knife edge A	0·121
30		knife edge B	2·038
31	Brass bar, $\frac{3}{4}$ inch thick	knife edge A	0·707
32		knife edge B	0·044
33		knife edge C	0·473
34		knife edge D	0·614

As the experiments, here alluded to, were made for the express purpose of detecting any discordance arising from the position of the knife edges on the agate planes, they were at first followed up (as far as each pendulum is concerned,) in immediate succession; alternately turning the face of the pendulum at the end of each experiment. It is needless to swell this paper with a detail of the whole of the experiments that were made on these occasions; but as the 10th case above enumerated (the iron bar No. 30, knife edge B,) affords so remarkable a discordance, I trust I may be excused for putting on record the steps of the process; by means of which the results may be verified at pleasure. The magnitude of the discordance (like the case already mentioned in page 461), was the cause of its detection, which may therefore be considered as accidental: but the discovery of the anomaly led me to suspect that it might also take place in other pendulums; which, from repeated trials, as above stated, I found to be the case. And this furnishes us with another proof of the propriety of varying such experiments in all manner of ways, in order to guard against the effect of any unsuspected source of error.

Results by turning the face of the Iron pendulum (No. 30).

Exp.	Knife edge B.	Knife edge *b*.	Exp.
205	86220·190	86220·999	206
207	20·346	23·499	208
209	20·433	21·818	210
213	21·002	21·976	211
215	20·524	23·309	212
216	20·129	22·967	214
218	20·574	21·791	217
219	20·302	22·338	222
220	20·247	22·450	223
221	20·473	22·401	224
227	20·504	22·881	225
228	20·362	23·077	226
229	20·962	23·033	230
Mean =	86220·465	86222·503	

It may here be stated that the knife edges of all the convertible pendulums in my possession are marked on *both* sides of the pendulum: on one side with the capital letters A, B, and on the reverse side with the small letters *a*, *b*. Therefore the column, in the above Table, designated as the knife edge B, denotes the results obtained when the side of the pendulum, marked B, is next to the observer: and the other column denotes the results when the pendulum is turned half round in azimuth, and consequently the side marked *b* is next to the observer. The mean difference in the results will be found, as already stated, equal to 2·038 vibrations in a mean solar day. If we compare the several results we shall find the partial differences somewhat greater than what generally occur in a regular series of experiments: but these have arisen from designedly varying the position of the knife edge on the agate plane, with a view to the discovery of the cause of the principal discordance; and which I can attribute to no other source than inequalities in the knife edge, or agate plane, or both; but which are not immediately perceptible to the eye. From a review of the whole question, however, it is clear that different experiments, even with the same pendulum, are not strictly comparable with each other, unless we can either ensure the perfect accuracy of the knife edges and planes, or provide a method of making the vibrations, in all cases, from the same part of the knife edge and from the same part of the plane: or, in other words, that the knife edge and plane shall, in all cases, touch each other at the same points of contact. This, I conceive, would not be difficult; and it must be attended to in all future experiments. We must deal with the experiments, already made, in the best manner we can*.

Correction for the Arc of Vibration.

In a recent volume of the Transactions of this Society† Captain SABINE has stated that the usual formula for the reduction of the vibrations of a pendulum, to indefinitely small arcs, is erroneous; inasmuch as it does not agree with the result of his observations, which require that the hitherto assumed corrections should, in the case of the convertible pendulum tried by him, when the heaviest end is below the axis of suspension, be multiplied by 1·12; and when it is above the axis of suspension, be multiplied by 1·40. As this view of the subject was somewhat at variance with what I had imagined to be the case in my own experiments, I determined on making a few trials in order to ascertain more minutely the difference which arises from the use of large and small arcs: and for this purpose I took the brass bar convertible pendulum No. 25 above enumerated. Two series were made (in the vacuum apparatus, and at about one inch pressure of the atmosphere,) on the knife edge A, and two on the knife edge B: and each of these series was divided into three portions; in the first of which, the arc was taken from about 1°·00 to about 0°·60; in the second, from 0°·60 to about 0°·38; and in the last, from 0°·38 to about 0°·20 and 0°·10. The first series on the knife edge A showed that the usual correction ought to be increased about $\frac{1}{10}$th; which accords very nearly with

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* Since this was written, I have caused my agate planes to be slightly *rounded*, so that a very fine thread of light can be seen under the knife edge, on each side of the small line where it touches the curve. By this method I have got rid of the discordancy in the pendulum No. 25—26, which is the only one I have yet tried in this way.

† Philosophical Transactions for 1831, page 461, &c.

Captain SABINE's determination: but the second series on the same knife edge indicated that it ought to be diminished by nearly the same quantity. I consider therefore these two series as neutralizing each other; and that the differences observed come within the errors of observation. With respect to the knife edge B, both series showed that the correction should be increased $\frac{1}{5}$th: which is only one half the amount indicated by Captain SABINE's experiments. Further inquiries therefore are requisite to clear up this point: not only as to the cause of the anomaly, whether it arises from a sliding of the knife edges on the agate planes (in which case, it may differ in different pendulums, and wholly vanish in M. BESSEL's mode of suspension); but also as to the accuracy of the assumed data on which the generally received formula is founded. When the arc is very large, the formula will not lead us to the true result: this has been already noticed by more than one author. But whether the difference arises from a defect in the formula, or from a sliding of the knife edges, or from the variable effect of the air on the pendulum, or from all three, remains still to be demonstrated. Should any experiments for determining this point be commenced, it would perhaps be better that the vacuum apparatus should not be used for the purpose: but that a heavy sphere, cylinder, or lens, suspended by a wire, be swung in free air, first on the knife edge, and afterwards over a steel cylinder; due care being taken, in the latter case, that the wire be *flat* at that portion of it which passes over the cylinder. A body of this kind will continue its vibrations for a sufficient length of time for such experiments: which was in fact the reason for adopting the vacuum apparatus for this purpose; but which may present difficulties of another kind; since it is difficult to prevent a leakage in the vacuum apparatus, which has a material effect on the arc of vibration; and moreover the proximity of the pendulum to the sides of the tube, when swinging in large arcs, may influence the results.

But, whatever be the cause of the discordancy, it is evident that in the present state of the subject we cannot strictly compare the results of experiments, where the arcs employed have been widely different. The initial arc ought in no case to exceed one degree: in my own experiments, I have generally commenced with an arc of about 0°·9 or 0°·8; but this I think is still too large, and were I again to undertake any delicate experiments on the pendulum, I should probably make the initial arc about half a degree only. In the experiments on the invariable pendulum made by the English, the initial arc has been about 1°·2 or 1°·3: but in those made by MM. Freycinet and Duperrey, the initial arc has sometimes amounted to upwards of 3½ degrees; and Mr. Rumker, in his experiments on the length of the seconds pendulum, has, in one instance, commenced with an arc of 11 degrees*.

On Captain Sabine's *recent determination of the length of the seconds pendulum at Greenwich.*

In the volume of the Philosophical Transactions just quoted, Captain Sabine has also given, what he considers, the true length of the seconds pendulum at Greenwich; and which he makes equal to 39·13734 inches, as deduced from his own observations there. It is not my intention to make any remark on those observations; which, indeed, appear to have been made with all due regard to accuracy: but, I trust I may be allowed, whilst treating on a subject of this kind, to express my dissent from the mode in which he has deduced the result in question. In all cases of the convertible pendulum, either the perfect synchronism of the two knife edges, or (which will answer the same purpose), the difference in the results of the two knife edges, ought to be well established, by an *equal weight* of evidence for *each* knife edge. This is indispensable: and, unless it be accomplished, the problem cannot be considered as strictly solved. Each knife edge is independent of the other; and each ought to have equal weight in the determination of the result. It is true that the knife edge A (or that position of the pendulum where the great weight is below the axis of suspension), will, in case of any difference, always give a result nearer to the true value than the knife edge B: but, the proper correction to be applied to make them synchronous, can only be determined by first giving to B an equal weight in the experiments. Now, perfect synchronism I consider unattainable; or, at all events, not worth the trouble it would cost to pursue it: since the small difference which arises, in these cases, will always enable us to apply the proper correction, from the known principles of the pendulum; and which are a more sure guide on such occasions than any partial determination of the correction from actual experiment, where, in these minute inquiries, the errors of observation are sure to baffle us in our object.

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* Memoirs of the Astronomical Society, vol. iii. page 289.

Captain Sabine however has preferred trusting to actual experiment for this minute correction: and, considering that the result shown by the knife edge A is the nearest to the truth, he has rested on the establishment of that result, without the requisite corroboration, by an *equal number* of trials, from the other knife edge; which are, in fact, equally essential to the establishment of the accuracy of the whole. Thus, he has swung the pendulum 188 hours on the knife edge A, and only 54 hours on the knife edge B. But, had this latter knife edge been employed during a longer period, it might probably have tended to correct the anomaly that occurs on the face of the observations. For, it appears that when the slider was moved about ·133 inch, it caused an *increase* of 0·10 vibration in a day, on the knife edge A; whilst it caused a *decrease* of 1·12 vibration on the knife edge B. But, this is contrary to the known principles of the pendulum, since the effect of a slider of this sort is to cause an alteration of the *same kind* in *each* knife edge, differing only in degree: the relative proportions of which may be ascertained by determining the distance of the centre of gravity from each knife edge *. In the state of the pendulum in question, when last used by Captain Sabine for the experiments here alluded to (the tail pieces being wholly removed), the distance of the centre of gravity from the knife edge A, I found by actual measurement, to be 26·23 inches; and from the knife edge B, 13·21 inches. We have therefore $\frac{26\cdot23}{13\cdot21} =$ 1·985 as the factor by which any alteration in the results of knife edge A must be multiplied, in order to show the corresponding alteration produced in the knife edge B: which will be *both* positive, or *both* negative. And if this is not shown by the experiment, we may reasonably suspect some error in the observations. Also, we have $\frac{13\cdot21}{26\cdot23 - 13\cdot21} = 1\cdot015$ as the factor by which the difference in the number of vibrations between A and B must be multiplied, to obtain the correction that should be applied to A, in order to ascertain the number of vibrations that the pendulum would make, if rendered perfectly synchronous: and which is the quantity to be used in determining the length of the seconds pendulum. In all such cases, however, it is presumed that the two knife edges are adjusted *very nearly* to synchronism. If we apply these principles to Captain SABINE's results, we shall have the following values for the number of vibrations if the pendulum were rendered perfectly synchronous.

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* The truth of this would have been shown, and the absolute amount easily determined, had Captain Sabine moved the slider through a larger space (one or two inches, for instance), so as to have produced a decided and powerful effect on the number of vibrations; sufficient to counterbalance the unavoidable errors of observation.

Slider.	A.	B.	(A—B)	If synchronous.
1·500	86069·00	86070·26	−1·26	86067·72
1·566	69·04	69·61	−0·57	68·46
1·633	69·10	69·14	−0·04	69·06

There is a difference, in these three values, of 6 and 7 tenths of a vibration; and if one is to be preferred to the other, it should be that which is the result of the greatest number of experiments, which appears to be the second value here given. But, they all want the requisite corroboration of the knife edge B.

Method of observing and of reducing the Observations.

Before I conclude this paper, it may be proper to say a few words on the method employed in making the experiments above alluded to, and of the data used in the reduction of the observations, in order that the circumstances, under which each experiment has been made, may fully appear, and that each step of the computations may be verified at pleasure.

The clock used for observing the coincidences is an excellent one made by MOLYNEUX, having a mercurial pendulum, with a long tail piece, furnished with two circular segments of gilt paper, which reflect a very brilliant light: the distance between these segments is variable at pleasure, in order to suit the size of the different pendulums under experiment. The rate of the clock is ascertained by a daily comparison with another clock (made by HARDY) regulated to sidereal time; the rate of which is determined by means of a 30-inch transit instrument. Both these clocks go very well; and with respect to the experiments detailed in this paper, which are merely *comparative*, do not afford the source of any appreciable error. The clock used with the experiments, and which I shall, for the sake of distinction, call the Pendulum-clock, has been in all the cases, regulated to mean solar time; except when used with the long cylindrical rod (No. 21) and with the long brass tube (No. 35—38); where it was necessary to alter the length of the clock pendulum in order to obtain convenient intervals for the coincidences. The daily rate of the pendulum clock has always been kept very low, for very obvious reasons: it has, in no case, exceeded $0^{s},80$ in a day*.

Let t denote the total interval of time, expressed in seconds, employed in any given series, as shown by the pendulum clock, making $(86400 + r)$ vibrations in a mean solar day; r being the daily rate of the clock, which will be *minus* when losing: and let n denote the number of coincidences (always including the first) that have taken place during that interval. Then will $\frac{t}{n-1}$ be the time of the *mean interval* of the coincidences, expressed in seconds of the clock, which I shall denote by m: and the number of vibrations (N) made by the pendulum of experiment, in a mean solar day, will be

$$\mathrm{N} = \frac{m \pm 2}{m}(86400 + r) = \frac{m \pm 2}{m}\,86400 + r\left(1 \pm \frac{2}{m}\right)$$

where the upper sign is to be taken when the pendulum of experiment goes faster than the clock; and the lower sign when it goes slower. All the pendulums enumerated in this paper, from No. 1 to No. 20 inclusive, go faster than the clock, and consequently the upper sign must be used in the computations. All the bar pendulums from No. 25 to No. 34, and the pendulums No. 40 and 41, go slower than the clock; and therefore the lower sign must be adopted in those cases. In all my reductions, however, I have made N equal to $\frac{m \pm 2}{m}\,86400$ only; and have afterwards applied $r\left(1 \pm \frac{2}{m}\right)$ as a separate correction for the rate of the clock. For the long cylindrical rod (No. 21) a special computation was made: and in the case of the cylindrical tube (No. 35—38) the pendulum clock was adjusted so as to make 90000 vibrations in a day: and the correction for the variation from that rate, applied afterwards.

In noting the coincidences I adopt the plan suggested by Professors AIRY and WHEWELL, and always observe the first and last disappearance and the first and last reappearance of the luminous disc: the mean of the four is the true time of the coincidence*. This is obviously the most correct mode of proceeding: more so than by observing only one disappearance, and one reappearance; and much more so than by observing the disappearance only or the reappearance only. It has also this convenience, that it obviates the necessity of attending to the minute adjustment of the diaphragm; and the eye is not in such case obliged to wander from one side of the pendulum to the other, doubting on which side the disappearance or reappearance will take place. I consider this part of the experiment as perfect: and that no appreciable error can occur when this mode of observing the coincidences is adopted†. In the detail of the experiments, the two moments of disappearance are written one over the other, with a line between, similar to a fractional quantity; and the same, with the reappearances: the mean of the four is annexed in the subsequent collateral column. Much has been said about the inutility of observing more than one of these phænomena; at which I must confess, I have been somewhat surprised‡. It is perhaps possible that, if the *same* person always made the observations, always under the *same* circumstances, always with the *same* magnitude of the disc, always with the *same* extent of the arc (and that not very small,) and always with precisely the *same* quantity of light, no great difference might be found in the results. But, as these are cases never likely to occur in practice, and from the nature of the subject must be perpetually varying, it is better to adopt a general and sure guide for determining the moment of coincidence: and had I not pursued this plan, I should in many instances have been led into considerable error.

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* A sudden change may sometimes be noticed between some of the series of observations: but this has occurred when the pendulum of experiment has been changed, and when it was necessary to stop the clock, in order to alter the luminous disc. In some cases where the variation in the daily rate has been an appreciable quantity, I have proportioned it, in the different experiments during the day, according to the intervals.

The arc of vibration has always been observed by means of a diagonal scale affixed to the clock case; and the divisions can be easily read off to the hundredth part of a degree. The scale is 7 inches distant from the pendulum, and a proper correction has, in each case, been applied to the arc for the proportion which this distance bears to the distance of the telescope from the pendulum. The values in the Table are the readings thus corrected.

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* The experiments with the long cylindrical rod (No. 21) form an exception: as, in this case, only *one* side of the rod could be seen in the vacuum tube.

† I have also adopted another suggestion of Professors Airy and Whewell, by removing the diaphragm from the inside of the telescope, and placing it between the pendulum of experiment and the clock pendulum. It is, in fact, attached to the clock case; and is not only capable of being moved in every direction, for the purpose of adjustment, but also of being enlarged or contracted, to suit the different pendulums employed.

‡ See Philosophical Transactions for 1826, page 4 &c.: and the same volume Part II. page 2 &c., containing Lieut. Foster's experiments on the pendulum. See also (*contra*) Captain Sabine's Account of Experiments, pages 217—233.

All the pendulums have been reduced to a common standard of temperature, which I have assumed equal to 62°. As I had no means of determining the expansion of the different metals, I have adopted such as I have considered most worthy of confidence. Any error arising from this source can be but trifling; as no considerable change of temperature has ever occurred during any two consecutive experiments. In the suspension by the iron and silver wire, I have taken into account the small piece of brass rod (about 1½ inch,) attached to the knife edge, and also the radius of the sphere. The following are the assumed rates of expansion for 1° of Fahrenheit's thermometer: viz.

Iron wire, &c. = ·000006666
Iron bar = ·000006850
Copper bar = ·000009444
Brass bar = ·000010000
Silver wire, &c. = ·000010600

The rate of expansion being denoted by e, the formula for the correction of the number of vibrations, on account of the temperature, will be

$$N \times \tfrac{1}{2} e (t^\circ - 62^\circ)$$

where t° denotes the mean height of the thermometer, during the interval of the coincidences. The mercurial pendulum (No. 39,) and the wooden rod pendulums (No. 40 and 41,) being compensation pendulums, do not require any correction for temperature.

For determining the temperature I have always used two excellent standard thermometers, made under Mr. Troughton's immediate inspection. These are placed inside the vacuum apparatus*; one of them on a level with the axis of suspension, and the other on a level with the centre of oscillation of the inclosed pendulum: the lower one can be read through the glass window of the tube. When the air is exhausted from the tube, I have, in computing the corrections for temperature, added 0·75 to the mean of the thermometers, to compensate for the effect produced on the thermometers by the removal of the pressure of the atmosphere; as indicated by Captain SABINE in the Philosophical Transactions for 1829, page 214: this being the amount by which these thermometers were also affected by such removal. In the detail of the experiments, inserted in the *Appendix* to this paper, the readings of the thermometer are given, without this correction. In recording the barometer, the correction for capillarity is always included: but when the vacuum tube is exhausted, a syphon gauge is employed to indicate the pressure of the atmosphere, and no correction is required.

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* In a few of the experiments, before I had contrived a method of suspending the lower thermometer in the inside of the tube, it was placed in a similar position (as to the centre of oscillation) on the outside. The inner thermometer however has, in all such cases, been used in the reductions; adding ·05 to the mean height: this being half the quantity by which the outer thermometer exceeded the other.

The subjoined *Appendix* consists of two Tables, in the first of which is given a detail of all the particulars (copied from the Observation-books,) requisite for deducing the corrections: and in the second of which is given the amount of those corrections under their respective heads. Table I. shows the time of the first and the last coincidence; the magnitude of the arc of vibration and the height of the barometer at those times respectively; the highest and lowest readings of the two thermometers, and the daily rate of the clock during the interval of each experiment: the number and date of which are always annexed. Table II. contains 1°. the corresponding number of each experiment in the preceding Table, for the sake of a convenient reference: 2°. the total interval of the experiment: 3°. the number of coincidences (minus unity) that have occurred: 4°. the mean interval expressed in seconds of the pendulum clock: 5°. the amount of the corrections for the arc, the thermometers, and the daily rate of the clock: and lastly the number of vibrations, N′ or N″, (according as the experiments were made in air or in vacuo,) in a mean solar day, exclusive of the correction for the pressure of the atmosphere, which is the quantity sought in the present inquiries. In the latter part of this Table, however, viz. from experiment 205 to 230 both inclusive, the correction for the barometer is added, and the last column then contains the true number of vibrations in a mean solar day, including the correction for the pressure of the atmosphere: for, these experiments are of a totally different kind, and are inserted to show the effect produced merely by reversing the face of the pendulum, as alluded to in page 467.

APPENDIX.

Table I.—*Detail of the Experiments.*

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s	°	°	°		s
No. 1. Platina Sphere.	1	Feb. 21	20 51 [illegible] 1 5 [illegible]	[illegible] [illegible]	57,5 10,0	0·77 ·29	38·4 39·0	38·4 39·1	30·256 30·228	—0,16
	2	Feb. 21	2 6 [illegible] 11 50 [illegible]	[illegible] [illegible]	56,5 6,0	·77 ·35	38·6 39·5	39·1 39·5	0·860 1·080	—0,18
	3	Feb. 21	11 51 [illegible] 22 16 [illegible]	[illegible] [illegible]	44,5 14,0	·83 ·34	39·7 38·8	39·5 38·5	1·080 1·250	—0,20
	4	Feb. 22	23 1 [illegible] 4 30 [illegible]	[illegible] [illegible]	39,5 45,2	·82 ·22	39·8 39·2	39·7 39·0	30·374 30·364	—0,25
	5	Feb. 23	20 9 [illegible] 1 13 [illegible]	[illegible] [illegible]	34,5 25,5	·81 ·24	38·6 38·5	38·5 38·4	30·396 30·344	—0,30
	6	Feb. 23	2 44 [illegible] 11 45 [illegible]	[illegible] [illegible]	27,0 42,5	·82 ·39	38·6 37·9	38·6 37·5	1·020 1·200	—0,35
	7	Feb. 23	11 46 [illegible] 21 37 [illegible]	[illegible] [illegible]	61,5 54,5	·82 ·34	38·0 37·0	37·6 37·0	1·210 1·360	—0,40
	8	Feb. 24	22 22 [illegible] 2 1 [illegible]	[illegible] [illegible]	30,5 54,5	·81 ·36	38·0 37·6	38·0 37·5	30·154 30·074	—0,45
No. 3. Small Brass Sphere.	9	Feb. 24	2 37 [illegible] 5 11 [illegible]	[illegible] [illegible]	20,5 53,5	·82 ·14	38·9 38·2	38·5 37·9	30·064 30·014	—0,50
	10	Feb. 24	5 53 [illegible] 12 41 [illegible]	[illegible] [illegible]	59,5 65,5	·82 ·14	37·5 37·0	37·3 36·7	1·080 1·250	—0,50
	11	Feb. 24	12 44 [illegible] 19 24 [illegible]	[illegible] [illegible]	17,5 30,0	·96 ·13	37·4 36·1	37·2 35·8	1·250 1·340	—0,50
	12	Feb. 25	20 12 [illegible] 22 14 [illegible]	[illegible] [illegible]	24,5 22,5	·96 ·23	37·4 37·0	37·5 36·7	30·040 30·090	—0,45
	13	Feb. 25	22 57 [illegible] 1 15 [illegible]	[illegible] [illegible]	13,5 39,0	·92 ·19	37·4 37·0	37·2 37·0	30·100 30·118	—0,43
	14	Feb. 25	2 47 [illegible] 11 57 [illegible]	[illegible] [illegible]	36,5 24,5	·94 ·10	36·7 37·1	36·9 37·2	0·930 1·090	—0,41
	15	Feb. 25	11 59 [illegible] 21 32 [illegible]	[illegible] [illegible]	46,0 63,0	·92 ·08	37·2 36·9	37·1 36·6	1·090 1·240	—0,40
	16	Feb. 26	22 15 [illegible] 0 25 [illegible]	[illegible] [illegible]	38,0 60,0	·96 ·19	38·0 37·9	38·1 37·9	30·210 30·184	—0,40

TABLE I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
No. 2. Small Lead Sphere.	17	March 5	0 51 [illegible] 3 42 [illegible]	[illegible] [illegible]	45,0 17,5	0·78 ·20	46·0 45·9	46·1 45·8	29·898 29·964	—0,06
	18	March 5	4 48 [illegible] 12 0 [illegible]	[illegible] [illegible]	33,5 61,5	·76 ·22	45·1 44·5	45·1 44·8	0·870 1·010	—0,06
	19	March 5	12 5 [illegible] 20 39 [illegible]	[illegible] [illegible]	24,5 49,5	·77 ·16	44·5 42·5	44·9 42·9	1·010 1·130	—0,03
	20	March 6	21 21 [illegible] 1 7 [illegible]	[illegible] [illegible]	11,0 49,5	·75 ·17	43·5 43·2	43·5 43·0	29·764 29·534	0,00
	21	March 6	1 11 [illegible] 4 11 [illegible]	[illegible] [illegible]	64,5 38,5	·77 ·19	43·3 43·5	43·0 43·5	29·534 29·384	0,00
	22	March 6	5 4 [illegible] 12 7 [illegible]	[illegible] [illegible]	25,5 34,0	·72 ·20	42·8 43·7	43·1 43·5	0·930 1·060	0,00
	23	March 6	12 9 [illegible] 20 25 [illegible]	[illegible] [illegible]	9,5 39,0	·78 ·19	43·7 42·6	43·5 42·2	1·060 1·190	0,00
	24	March 7	21 3 [illegible] 0 2 [illegible]	[illegible] [illegible]	18,5 50,5	·80 ·20	43·8 43·5	43·7 43·2	29·422 29·418	0,00
No. 4. Small Ivory Sphere.	25	May 10	0 31 [illegible] 1 15 [illegible]	[illegible] [illegible]	28,5 35,5	·77 ·12	53·5 53·5	53·3 53·3	30·420 30·428	—0,43
	26	May 10	2 17 [illegible] 4 1 [illegible]	[illegible] [illegible]	55,5 35,5	·77 ·19	52·6 52·9	52·5 52·8	1·040 1·140	—0,43
	27	May 11	20 16 [illegible] 22 0 [illegible]	[illegible] [illegible]	55,5 47,0	·78 ·16	50·2 50·5	49·8 50·5	2·070 2·150	—0,46
	28	May 11	22 58 [illegible] 23 49 [illegible]	[illegible] [illegible]	35,5 55,2	·78 ·12	51·5 51·7	51·7 51·7	30·310 30·284	—0,46
	29	May 12	18 52 [illegible] 19 43 [illegible]	[illegible] [illegible]	43,5 68,5	·78 ·12	51·5 51·5	51·1 51·3	30·014 30·004	—0,48
	30	May 12	20 26 [illegible] 22 42 [illegible]	[illegible] [illegible]	58,5 73,0	·79 ·12	50·6 51·0	50·1 50·8	1·000 1·130	—0,48
	31	May 12	22 46 [illegible] 1 2 [illegible]	[illegible] [illegible]	37,5 61,0	·79 ·12	51·0 51·4	50·8 51·4	1·130 1·280	—0,48
	32	May 12	1 57 [illegible] 2 48 [illegible]	[illegible] [illegible]	41,5 74,5	·78 ·11	52·4 52·5	52·5 52·5	29·910 29·894	—0,48
No. 6. Large Brass Sphere.	33	Feb. 13	23 5 [illegible] 2 0 [illegible]	[illegible] [illegible]	44,5 38,0	·88 ·34	42·0 41·7	42·0 41·4	30·118 30·100	—0,52
	34	Feb. 13	3 17 [illegible] 12 1 [illegible]	[illegible] [illegible]	56,5 32,0	·96 ·36	41·0 40·7	41·4 40·5	0·910 1·050	—0,56
	35	Feb. 13	12 5 [illegible] 20 48 [illegible]	[illegible] [illegible]	48,5 53,0	·87 ·27	40·8 40·1	40·5 39·9	1·050 1·150	—0,60
	36	Feb. 14	21 38 [illegible] 2 19 [illegible]	[illegible] [illegible]	38,5 56,0	·91 ·16	41·3 40·7	41·2 40·5	30·088 30·060	—0,63
	37	Feb. 14	2 32 [illegible] 6 44 [illegible]	[illegible] [illegible]	18,5 51,0	·96 ·19	41·0 40·6	40·8 40·5	30·058 30·064	—0,63
	38	Feb. 14	11 12 [illegible] 20 32 [illegible]	[illegible] [illegible]	46,0 45,5	·96 ·32	40·1 37·9	39·6 37·3	0·960 1·090	—0,63
	39	Feb. 15	*20 32 [illegible] 23 47 [illegible]	[illegible] [illegible]	45,5 64,5	·32 ·19	37·9 37·5	37·3 37·0	1·090 1·140	—0,63
	40	Feb. 15	0 37 [illegible] 3 32 [illegible]	[illegible] [illegible]	57,0 21,0	·96 ·34	38·5 38·2	38·4 38·0	30·050 30·028	—0,63

* The preceding series continued.

Table I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s	°	°	°		s
No. 7. Large Ivory Sphere.	41	Feb. 16	23 55 [illegible] 1 1 [illegible]	[illegible] [illegible]	43,0 54,0	0·89 ·19	35·8 35·8	35·7 35·8	29·804 29·780	−0,60
	42	Feb. 17	20 17 [illegible] 22 50 [illegible]	[illegible] [illegible]	46,0 35,0	·96 ·22	36·0 36·4	36·0 36·4	0·890 0·940	−0,64
	43	Feb. 17	22 53 [illegible] 1 26 [illegible]	[illegible] [illegible]	42,5 37,5	·96 ·21	36·4 36·9	36·4 37·0	0·940 1·000	−0,66
	44	Feb. 17	2 3 [illegible] 3 9 [illegible]	[illegible] [illegible]	21,5 43,5	·96 ·15	38·1 38·1	38·4 38·3	29·842 29·864	−0,68
	45	Feb. 18	20 34 [illegible] 21 40 [illegible]	[illegible] [illegible]	39,5 73,0	·94 ·16	39·4 39·5	39·2 39·5	30·202 30·220	−0,72
	46	Feb. 18	22 35 [illegible] 1 16 [illegible]	[illegible] [illegible]	40,5 57,5	·98 ·20	38·7 39·6	38·6 39·8	0·920 1·000	−0,73
	47	Feb. 20	22 30 [illegible] 1 11 [illegible]	[illegible] [illegible]	23,0 56,0	·98 ·19	37·9 37·9	37·5 37·6	1·540 1·570	−0,80
	48	Feb. 20	1 58 [illegible] 3 4 [illegible]	[illegible] [illegible]	34,5 63,5	·96 ·17	38·8 38·8	38·7 38·7	30·326 30·324	−0,80
No. 5. Large Lead Sphere.	49	March 8	0 4 [illegible] 5 25 [illegible]	[illegible] [illegible]	34,5 27,5	·86 ·21	42·9 42·8	43·0 42·5	29·606 29·714	−0,23
	50	March 8	6 4 [illegible] 12 31 [illegible]	[illegible] [illegible]	65,0 26,0	·82 ·47	42·0 41·5	42·0 41·0	1·000 1·170	−0,23
	51	March 8	12 33 [illegible] 20 3 [illegible]	[illegible] [illegible]	33,5 34,5	·84 ·41	41·7 39·7	41·4 39·0	1·170 1·270	−0,22
	52	March 9	20 42 [illegible] 1 6 [illegible]	[illegible] [illegible]	14,0 60,0	·80 ·29	40·6 40·2	40·4 39·8	30·054 30·124	−0,22
	53	March 9	12 5 [illegible] 19 48 [illegible]	[illegible] [illegible]	28,0 38,5	·86 ·10	41·5 39·0	41·1 39·0	30·340 30·398	−0,22
	54	March 10	20 28 [illegible] 4 51 [illegible]	[illegible] [illegible]	7,0 11,5	·87 ·39	38·9 40·0	38·5 40·0	0·950 1·150	−0,22
	55	March 10	4 52 [illegible] 12 43 [illegible]	[illegible] [illegible]	25,5 55,0	·78 ·40	40·0 40·2	40·0 39·9	1·150 1·270	−0,23
	56	March 11	21 15 [illegible] 2 46 [illegible]	[illegible] [illegible]	59,5 63,5	·83 ·20	40·0 40·0	39·7 39·8	30·208 30·126	−0,24
No. 9. Large Ivory Sphere.	57	April 9	19 40 [illegible] 20 52 [illegible]	[illegible] [illegible]	18,0 96,5	·87 ·11	49·5 49·5	49·0 49·4	30·254 30·260	+0,20
	58	April 9	21 59 [illegible] 0 37 [illegible]	[illegible] [illegible]	10,5 25,0	·91 ·21	48·8 49·5	48·5 49·5	1·080 1·180	+0,20
	59	April 9	0 41 [illegible] 3 37 [illegible]	[illegible] [illegible]	46,0 49,0	·91 ·14	49·5 50·7	49·5 50·6	1·180 1·280	+0,20
	60	April 9	4 10 [illegible] 5 23 [illegible]	[illegible] [illegible]	63,0 83,0	·89 ·11	52·0 52·0	52·2 52·0	30·230 30·232	+0,20
No. 8. Large Lead Sphere.	61	April 10	20 3 [illegible] 23 17 [illegible]	[illegible] [illegible]	60,5 46,0	·87 ·20	48·8 49·0	48·3 48·5	30·268 30·244	+0,23
	62	April 10	0 10 [illegible] 5 26 [illegible]	[illegible] [illegible]	57,5 54,0	·90 ·38	48·3 50·0	48·3 50·0	0·930 1·170	+0,23
	63	April 10	5 28 [illegible] 12 18 [illegible]	[illegible] [illegible]	12,0 15,5	·90 ·36	50·0 49·5	50·0 49·0	1·170 1·430	+0,23
	64	April 11	20 3 [illegible] 23 16 [illegible]	[illegible] [illegible]	17,5 14,0	·90 ·19	48·1 48·5	47·5 48·3	30·214 30·184	+0,20

TABLE I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Re-appearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s					s
No. 10. 2-inch Brass Cylinder.	65	March 21	21 37 [illegible] 1 44 [illegible]	[illegible] [illegible]	12,0 50,0	0°97 ·23	47°1 47·7	47°1 47·7	30·060 30·114	+0,60
	66	March 21	2 47 [illegible] 11 44 [illegible]	[illegible] [illegible]	39,0 37,5	·90 ·38	47·2 48·5	47·2 48·3	0·790 0·960	+0,60
	67	March 21	11 47 [illegible] 20 2 [illegible]	[illegible] [illegible]	22,5 59,5	·93 ·38	48·7 48·0	48·5 47·6	0·960 1·060	+0,57
	68	March 22	20 49 [illegible] 0 14 [illegible]	[illegible] [illegible]	34,0 35,5	·96 ·29	48·8 49·0	48·6 49·0	30·178 30·164	+0,57
No. 11. 2-inch Brass Cylinder.	69	April 16	21 5 [illegible] 23 51 [illegible]	[illegible] [illegible]	18,0 55,0	·83 ·24	55·0 54·5	54·5 54·3	30·056 30·050	+0,50
	70	April 16	0 54 [illegible] 4 46 [illegible]	[illegible] [illegible]	41,5 37,5	·86 ·43	54·0 55·2	54·0 55·3	1·700 1·880	+0,50
	71	April 16	4 49 [illegible] 8 53 [illegible]	[illegible] [illegible]	14,0 49,5	·85 ·42	55·2 55·5	55·3 55·3	1·880 2·060	+0,50
	72	April 16	9 33 [illegible] 11 54 [illegible]	[illegible] [illegible]	39,0 70,2	·83 ·27	56·5 56·0	56·6 55·5	30·086 30·080	+0,50
No. 12. 2-inch Brass Cylinder.	73	April 17	20 52 [illegible] 22 52 [illegible]	[illegible] [illegible]	60,0 48,0	·89 ·32	53·5 53·2	53·1 53·0	30·074 30·056	+0,52
	74	April 17	0 0 [illegible] 4 37 [illegible]	[illegible] [illegible]	19,0 43,5	·91 ·46	52·3 53·1	52·0 53·2	1·190 1·370	+0,52
	75	April 17	4 39 [illegible] 9 4 [illegible]	[illegible] [illegible]	29,5 54,5	·90 ·46	53·1 53·5	53·2 53·3	1·370 1·540	+0,52
	76	April 17	9 45 [illegible] 11 31 [illegible]	[illegible] [illegible]	30,0 76,0	·90 ·38	54·4 53·9	54·5 53·5	29·972 29·946	+0,52
No. 13. 2-inch Brass Cylinder.	77	April 18	19 45 [illegible] 23 50 [illegible]	[illegible] [illegible]	46,5 46,5	·86 ·11	52·4 53·0	52·0 53·0	29·810 29·726	+0,60
	78	April 18	1 15 [illegible] 11 30 [illegible]	[illegible] [illegible]	36,5 35,0	·96 ·20	52·8 54·5	53·0 54·2	1·060 1·480	+0,60
	79	April 18	11 32 [illegible] 19 44 [illegible]	[illegible] [illegible]	39,5 66,5	·96 ·25	54·5 53·8	54·3 53·5	1·480 1·800	+0,60
	80	April 19	20 35 [illegible] 23 0 [illegible]	[illegible] [illegible]	34,5 30,0	·96 ·33	54·5 54·5	54·5 54·3	29·664 29·702	+0,60
No. 18. Hollow Brass Cylinder.	81	March 14	2 42 [illegible] 3 59 [illegible]	[illegible] [illegible]	39,5 45,5	·96 ·29	45·0 45·0	45·5 45·0	29·394 29·372	+0,49
	82	March 14	6 56 [illegible] 11 3 [illegible]	[illegible] [illegible]	48,0 68,5	·98 ·34	44·3 44·5	44·1 44·4	1·030 1·160	+0,49
	83	March 15	20 39 [illegible] 0 37 [illegible]	[illegible] [illegible]	39,5 61,5	·87 ·29	43·0 43·0	42·6 42·9	1·250 1·290	+0,45
	84	March 15	1 27 [illegible] 3 22 [illegible]	[illegible] [illegible]	14,5 51,5	·96 ·20	44·1 44·0	44·1 44·0	29·574 29·628	+0,45
No. 15. Hollow Brass Cylinder.	85	March 16	20 36 [illegible] 22 8 [illegible]	[illegible] [illegible]	12,5 64,5	·97 ·18	42·0 41·8	41·5 41·5	29·840 29·836	+0,58
	86	March 16	22 48 [illegible] 1 56 [illegible]	[illegible] [illegible]	10,5 49,5	·87 ·36	40·8 41·4	40·5 41·3	0·950 1·090	+0,58
	87	March 16	1 58 [illegible] 5 7 [illegible]	[illegible] [illegible]	27,5 22,0	·96 ·40	41·4 41·8	41·3 41·8	1·090 1 150	+0,58
	88	March 16	7 42 [illegible] 9 7 [illegible]	[illegible] [illegible]	52,0 63,0	·94 ·17	42·8 43·0	42·5 43·0	29·672 29·610	+0,58

TABLE I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s	°	°	°		s
No. 16. Hollow Brass Cylinder.	89	March 17	20 16 [illegible] 22 15 [illegible]	[illegible] [illegible]	64,5 43,5	0·90 ·15	44·6 45·4	44·5 45·4	29·448 29·464	+0,66
	90	March 17	0 58 $\frac{9}{10}$ 5 4 [illegible]	[illegible] [illegible]	15,0 60,5	·98 ·28	45·6 46·5	46·0 46·4	1·990 2·060	+0,66
	91	March 17	5 7 [illegible] 8 54 [illegible]	[illegible] [illegible]	18,5 68,5	1·01 0·31	46·5 46·5	46·4 46·0	2·060 2·090	+0,66
	92	March 17	9 51 [illegible] 11 49 [illegible]	[illegible] [illegible]	33,0 81,5	·97 ·16	47·3 47·0	47·0 46·4	29·564 29·564	+0,66
No. 17. Hollow Brass Cylinder.	93	March 19	20 28 [illegible] 22 8 [illegible]	[illegible] [illegible]	33,0 69,5	·97 ·15	45·0 45·0	44·5 44·8	29·882 29·872	+0,43
	94	March 19	22 54 $\frac{7}{8}$ 1 57 $\frac{4}{5}$	[illegible] [illegible]	12,0 9,0	·98 ·41	44·2 45·1	44·0 45·3	1·200 1·290	+0,43
	95	March 20	0 33 $\frac{9}{10}$ 4 2 [illegible]	[illegible] [illegible]	14,0 48,5	·98 ·37	46·1 46·6	46·0 46·6	1·570 1·640	+0,54
	96	March 20	4 37 [illegible] 6 10 [illegible]	[illegible] [illegible]	4,5 38,0	·98 ·27	47·8 47·8	48·0 47·8	29·806 29·864	+0,54
No. 14. Solid Lead Cylinder.	97	April 4	12 16 [illegible] 20 1 [illegible]	[illegible] [illegible]	8,5 51,5	·78 ·11	55·8 54·4	55·5 54·0	30·562 30·520	+0,50
	98	April 5	20 54 [illegible] 4 33 [illegible]	[illegible] [illegible]	64,5 42,0	·77 ·47	53·6 56·5	53·6 56·8	0·950 1·190	+0,60
	99	April 5	* 4 33 [illegible] 10 54 [illegible]	[illegible] [illegible]	42,0 59,0	·47 ·33	56·5 57·2	56·8 57·0	1·190 1·360	+0,60
	100	April 5	11 52 [illegible] 19 54 [illegible]	[illegible] [illegible]	34,0 47,5	·77 ·10	58·0 55·5	58·0 55·5	30·464 30·446	+0,60
No. 19. Hollow Brass Cylinder.	101	April 13	19 36 $\frac{7}{10}$ 20 48 [illegible]	[illegible] [illegible]	13,5 54,5	·96 ·26	48·1 48·2	47·6 48·0	29·819 29·829	+0,20
	102	April 13	22 1 [illegible] 1 7 [illegible]	[illegible] [illegible]	54,5 17,5	·98 ·36	47·6 48·9	47·5 48·9	1·130 1·340	+0,20
	103	April 14	19 20 [illegible] 22 32 [illegible]	[illegible] [illegible]	23,5 49,5	·97 ·37	48·9 48·9	48·4 48·7	1·070 1·260	+0,30
	104	April 14	23 30 [illegible] 0 59 [illegible]	[illegible] [illegible]	23,5 37,5	·98 ·22	50·2 50·5	50·3 50·4	30·176 30·178	+0,30
No. 20. Lead Lens.	105	March 22	3 32 [illegible] 7 38 [illegible]	[illegible] [illegible]	38,5 57,5	·77 ·25	51·1 51·0	51·5 50·6	30·132 30·114	+0,52
	106	March 22	11 3 [illegible] 20 7 [illegible]	[illegible] [illegible]	39,5 36,5	·88 ·26	50·4 49·9	50·2 49·5	1·030 1·160	+0,52
	107	March 23	1 18 $\frac{3}{4}$ 11 59 [illegible]	[illegible] [illegible]	10,5 58,0	·78 ·19	50·2 50·0	50·2 49·4	1·230 1·380	+0,44
	108	March 24	20 22 $\frac{1}{4}$ 0 27 [illegible]	[illegible] [illegible]	10,0 22,5	·77 ·24	47·8 47·2	47·0 46·8	29·870 29·878	+0,44
No. 21. Long Cylindrical Rod.	109	April 26	20 26 54 22 18 30	55 50	54,5 40,0	·87 ·06	51·6 51·4	51·5 51·0	29·800 29·794	The clock pendulum altered, so that it made 86045·291 seconds in a mean solar day.
	110	April 26	23 10 14 2 33 34	15 39	14,5 36,5	·90 ·21	50·4 50·5	50·0 50·4	0·960 1·140	
	111	April 26	2 35 6 5 36 58	7 65	6,5 61,5	·89 ·21	50·5 50·6	50·4 50·5	1·140 1·280	
	112	April 26	6 15 4 8 17 6	5 33	4,5 19,5	·89 ·06	51·6 51·3	51·9 51·0	29·844 29·864	

* The preceding series continued.

TABLE I.—Continued.

No. 25—26. Brass bar, $\frac{3}{8}$ inch thick.

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
113	A	August 7	18 28 [illegible]	[illegible]	51,5	0·90	69·6	69·0	1·020	+0,23
			2 33 [illegible]	[illegible]	14,0	·31	70·9	70·0	1·150	
114	A	August 7	3 28 [illegible]	[illegible]	35,0	·98	71·4	71·0	29·698	+0,23
			7 2 [illegible]	[illegible]	32,5	·14	71·3	71·0	29·730	
115	B	August 8	19 27 [illegible]	[illegible]	31,5	·99	68·5	68·4	29·852	+0,23
			22 19 [illegible]	[illegible]	61,0	·23	68·9	68·6	29·876	
116	B	August 8	23 5 [illegible]	[illegible]	17,0	1·00	69·3	68·0	0·950	+0,20
			3 55 [illegible]	[illegible]	12,0	0·51	71·5	70·0	1·130	
117	B	August 8	4 15 [illegible]	[illegible]	51,5	·97	71·8	70·3	* 0·650	+0,20
			12 7 [illegible]	[illegible]	51,0	·35	71·4	70·6	0·830	
118	B	August 9	20 38 [illegible]	[illegible]	28,5	·98	70·2	70·0	29·976	+0,15
			23 44 [illegible]	[illegible]	42,7	·19	71·0	70·5	29·980	
119	A	August 21	23 52 [illegible]	[illegible]	29,5	·98	65·0	64·0	1·290	–0,18
			19 33 [illegible]	[illegible]	149,0	·03	63·5	63·2	1·510	
120	A	August 22	20 29 [illegible]	[illegible]	14,0	·97	63·8	64·1	30·314	–0,18
			0 49 [illegible]	[illegible]	62,0	·08	64·8	64·5	30·308	
121	A	August 22	0 51 [illegible]	[illegible]	39,0	·97	64·8	64·4	30·308	–0,18
			4 57 [illegible]	[illegible]	34,0	·09	66·2	65·6	30·274	
							† Upper.	† Lower.		
122	*a*	Dec. 5	2 6 [illegible]	[illegible]	52,0	·79	49·0	48·8	29·764	+0,43
			3 6 [illegible]	[illegible]	55,0	·42	48·9	48·5	29·748	
123	*a*	Dec. 5	6 50 [illegible]	[illegible]	24,0	1·01	48·7	48·5	29·680	+0,45
			8 49 [illegible]	[illegible]	88,5	0·31	48·8	48·5	29·650	
124	*a*	Dec. 5	10 22 [illegible]	[illegible]	60,5	·98	48·6	48·2	1·060	+0,50
			20 46 [illegible]	[illegible]	62,0	·18	47·4	47·4	1·600	
125	*a*	Dec. 6	21 20 [illegible]	[illegible]	11,5	·99	48·6	48·6	29·464	+0,52
			23 34 [illegible]	[illegible]	79,5	·25	48·4	48·2	29·478	
126	*b*	Dec. 7	23 50 [illegible]	[illegible]	58,0	·99	50·0	49·8	28·934	+0,63
			1 5 [illegible]	[illegible]	31,0	·45	50·1	50·1	28·924	
127	*b*	Dec. 7	1 14 [illegible]	[illegible]	8,5	·99	50·1	50·1	28·924	+0,70
			4 12 [illegible]	[illegible]	66,0	·19	50·5	50·5	29·074	
128	*b*	Dec. 7	7 33 [illegible]	[illegible]	44,0	1·01	50·0	49·9	0·890	+0,70
			21 37 [illegible]	[illegible]	29,0	0·10	49·9	49·7	1·560	
129	*b*	Dec. 8	22 30 [illegible]	[illegible]	50,0	1·00	50·9	50·7	29·198	+0,70
			1 29 [illegible]	[illegible]	27,0	0·18	51·0	50·9	29·202	
130	*b*	Dec. 8	1 55 [illegible]	[illegible]	14,5	1·01	51·1	51·0	29·206	+0,70
			5 8 [illegible]	[illegible]	41,0	0·16	51·5	51·3	29·260	
131	B	Dec. 9	21 2 [illegible]	[illegible]	33,5	1·01	53·3	52·9	29·222	+0,70
			23 0 [illegible]	[illegible]	63,0	0·29	53·1	52·9	29·304	
132	B	Dec. 9	23 8 [illegible]	[illegible]	25,5	1·01	53·1	52·9	29·304	+0,70
			1 21 [illegible]	[illegible]	41,0	0·27	53·3	53·0	29·244	
133	B	Dec. 9	2 12 [illegible]	[illegible]	38,0	1·00	52·5	52·6	0·910	+0,70
			‡ 20 56 [illegible]	[illegible]	60,5	0·03	51·4	51·1	1·760	
134	B	Dec. 10	21 31 [illegible]	[illegible]	6,0	1·00	53·0	52·9	29·454	+0,71
			0 58 [illegible]	[illegible]	61,5	0·14	52·5	52·5	29·534	

* Pumped out a little more air. † Both inside. ‡ Observed only on one side of the pendulum.

Table I.—Continued.

No. 25—26. Brass bar, $\frac{3}{8}$ inch thick (continued).

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s					s
135	B	Dec. 10	1 14 [illegible] 4 42 [illegible]	[illegible] [illegible]	47,5 46,0	1°·01 0·13	52°·6 52·7	52°·5 52·6	29·546 29·626	+0,72
136	A	Dec. 12	21 20 [illegible] 0 33 [illegible]	[illegible] [illegible]	61,0 43,5	·99 ·15	53·0 52·8	53·0 52·6	29·240 29·086	+0,70
137	A	Dec. 12	0 38 [illegible] 2 21 [illegible]	[illegible] [illegible]	23,0 56,5	1·00 0·36	52·8 52·9	52·6 52·9	29·086 29·064	+0,70
138	A	Dec. 12	2 24 [illegible] 5 7 [illegible]	[illegible] [illegible]	34,0 29,5	·99 ·19	52·9 53·1	52·9 53·0	29·164 29·164	+0,66
139	A	Dec. 12	9 6 [illegible] 22 29 [illegible]	[illegible] [illegible]	19,5 71,5	·99 ·12	52·7 51·6	52·6 51·3	0·770 1·470	+0,64
140	A	Dec. 13	23 44 [illegible] 2 12 [illegible]	[illegible] [illegible]	11,0 23,5	·98 ·20	52·6 52·6	52·5 52·5	29·492 29·512	+0,62
141	A	Dec. 13	2 16 [illegible] 4 44 [illegible]	[illegible] [illegible]	60,0 77,0	·98 ·20	52·6 52·6	52·5 52·5	29·502 29·534	+0,58

No. 31—34. Brass bar, $\frac{3}{4}$ inch thick.

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Outside.	Inside.		
			h m s	s	s					s
142	A	Nov. 15	1 59 [illegible] 4 53 [illegible]	[illegible] [illegible]	21,5 15,0	0°·94 ·27	44°·5 43·7	44°·0 43·6	29·394 29·374	+0,15
143	A	Nov. 15	11 49 [illegible] 2 6 [illegible]	[illegible] [illegible]	13,0 31,0	·91 ·14	42·1 40·4	41·5 39·5	1·330 2·080	+0,15
144	A	Nov. 16	4 46 [illegible] 7 57 [illegible]	[illegible] [illegible]	14,0 25,0	1·00 0·23	40·5 40·8	40·3 40·5	29·419 29·460	+0,15
145	B	Nov. 16	8 19 [illegible] 10 2 [illegible]	[illegible] [illegible]	48,5 47,0	1·03 0·44	42·5 40·8	41·3 40·6	29·460 29·494	+0,15
146	B	Nov. 16	11 27 [illegible] 20 57 [illegible]	[illegible] [illegible]	38,5 48,0	1·02 0·33	40·9 39·1	40·0 38·0	1·140 1·620	+0,15
147	B	Nov. 17	21 23 [illegible] 0 5 [illegible]	[illegible] [illegible]	35,5 70,5	·98 ·31	39·6 39·0	39·8 39·0	29·594 29·574	+0,10
148	D	Nov. 17	11 34 [illegible] 0 38 [illegible]	[illegible] [illegible]	35,5 43,5	·83 ·17	38·0 37·3	37·4 36·3	1·390 2·030	+0,10
149	D	Nov. 18	1 46 [illegible] 4 29 [illegible]	[illegible] [illegible]	12,5 57,0	1·01 0·27	38·0 37·5	37·5 37·3	29·740 29·782	0,00
150	D	Nov. 19	21 25 [illegible] 3 8 [illegible]	[illegible] [illegible]	53,5 61,5	·99 ·09	37·4 39·2	36·8 38·2	29·446 29·500	+0,20
151	C	Nov. 19	3 44 [illegible] 6 24 [illegible]	[illegible] [illegible]	39,5 60,0	1·01 0·33	39·9 39·5	38·8 39·0	29·520 29·582	+0,20
152	C	Nov. 19	12 16 [illegible] 2 7 [illegible]	[illegible] [illegible]	67,0 82,5	1·02 0·15	39·5 38·4	38·8 37·4	1·920 2·740	+0,20
153	C	Nov. 21	20 47 [illegible] 23 59 [illegible]	[illegible] [illegible]	62,0 21,5	1·00 0·27	41·9 43·5	40·9 42·4	29·672 29·732	+0,30

TABLE I.—Continued.

No. 31—34. Brass bar, ¾ inch thick (continued).

No.	Knife edge.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers, both inside. Upper.	Thermometers, both inside. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
154	A	Dec. 15	21 19 [illegible] 0 43 [illegible]	[illegible] [illegible]	14,0 15,0	0·82 0·19	48·3 47·8	47·8 47·5	29·800 29·790	+0,40
155	A	Dec. 15	1 51 [illegible] 20 44 [illegible]	[illegible] [illegible]	6,5 58,5	1·01 0·08	47·3 45·8	47·0 45·6	1·100 2·020	+0,40
156	A	Dec. 16	22 9 [illegible] 1 17 [illegible]	[illegible] [illegible]	23,5 58,5	1·04 0·05	46·7 46·4	46·6 46·2	29·832 29·736	+0,40
157	B	Dec. 17	1 45 [illegible] 4 40 [illegible]	[illegible] [illegible]	49,0 49,5	1·06 0·29	46·6 46·5	46·5 46·2	29·796 29·784	+0,45
158	B	Dec. 17	4 42 [illegible] 7 52 [illegible]	[illegible] [illegible]	55,5 28,5	1·04 0·27	46·6 46·5	46·4 46·2	29·784 29·734	+0,45
159	B	Dec. 18	12 6 [illegible] 2 7 [illegible]	[illegible] [illegible]	22,5 78,5	0·99 0·16	47·0 46·4	46·8 46·4	1·300 2·100	+0,45
160	B	Dec. 18	3 1 [illegible] 5 55 [illegible]	[illegible] [illegible]	11,5 34,5	0·98 0·29	47·8 47·5	47·8 47·2	29·420 29·444	+0,45
161	D	Dec. 19	20 33 [illegible] 0 57 [illegible]	[illegible] [illegible]	55,5 28,5	0·99 0·13	46·0 45·7	45·9 45·5	29·564 29·624	+0,40
162	D	Dec. 19	2 26 [illegible] 21 3 [illegible]	[illegible] [illegible]	21,5 61,0	0·88 0·09	45·1 43·8	45·0 43·6	0·960 2·080	+0,40
163	D	Dec. 20	22 34 [illegible] 1 29 [illegible]	[illegible] [illegible]	8,5 67,5	1·01 0·27	44·5 44·4	44·3 44·1	29·776 29·738	+0,40
164	C	Dec. 20	1 57 [illegible] 4 51 [illegible]	[illegible] [illegible]	65,5 68,5	1·00 0·29	45·0 44·5	44·5 44·4	29·728 29·686	+0,40
165	C	Dec. 20	6 50 [illegible] 20 30 [illegible]	[illegible] [illegible]	17,5 108,0	0·99 0·17	44·1 43·7	43·9 43·6	1·050 1·790	+0,40
166	C	Dec. 21	21 19 [illegible] 0 29 [illegible]	[illegible] [illegible]	21,0 26,0	0·86 0·23	45·1 44·9	45·1 44·7	29·670 29·720	+0,40

No. 35—38. Brass tube.

No.	Plane.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			No. 3. Knife edge. No. 8. Diameter.							
			h m s	s	s	°	°			s
167	c	March 15	9 0 [illegible] 11 13 [illegible]	[illegible] [illegible]	15,5 16,0	0·98 ·51	46·5 46 1		1·140 1·350	0,00
168	c	March 15	11 29 [illegible] 12 28 [illegible]	[illegible] [illegible]	44,5 39,5	·99 ·54	46·5 46·6		29·680 29·660	0,00
169	A	March 16	6 58 [illegible] 8 48 [illegible]	[illegible] [illegible]	48,5 36,0	·94 ·13	48 6 49·0		29·660 29·700	0,00
170	A	March 16	9 16 [illegible] 10 51 [illegible]	[illegible] [illegible]	8,5 15,0	·96 ·64	49·8 50·0		0·950 1·120	0,00
171	C	March 17	7 13 [illegible] 9 25 [illegible]	[illegible] [illegible]	14,5 69,5	·98 ·10	52·9 52·6		29·880 29·860	0,00
172	C	March 17	9 49 [illegible] 13 23 [illegible]	[illegible] [illegible]	12,5 43,5	·99 ·42	53·0 53·8		0·850 0·990	0,00
173	a	March 18	8 18 [illegible] 11 35 [illegible]	[illegible] [illegible]	27,5 30,5	·98 ·43	50·8 50 6		0·880 1·000	0,00
174	a	March 18	11 56 [illegible] 13 20 [illegible]	[illegible] [illegible]	55,5 15,5	·98 ·26	51·0 51·0		30·200 30·200	0,00

TABLE I.—Continued.

No. 35—38. Brass tube (continued).

No.	Plane.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Outside.	Inside.		
		No. 1. Knife edge.	No. 3. Diameter.	Adjustment altered.						
			h m s	s	s	°	°	°		s
175	A	May 17	11 30 [illegible]	[illegible]	31,0	0·97	61·4	60·5	1·500	—0,30
			14 58 [illegible]	[illegible]	27,5	·32	62·5	61·7	1·820	
176	A	May 17	15 11 [illegible]	[illegible]	45,0	·97	62·9	62·0	1·580	—0,30
			21 0 [illegible]	[illegible]	43,5	·13	61·5	61·0	2·100	
177	A	May 18	6 41 [illegible]	[illegible]	21,5	·95	60·1	60·0	30·040	—0,30
			8 47 [illegible]	[illegible]	40,5	·11	61·0	60·8	30·010	
178	A	May 18	8 52 [illegible]	[illegible]	51,5	·91	61·0	60·8	30·010	—0,30
			11 24 [illegible]	[illegible]	19,0	·06	62·0	61·9	29·990	
179	C	May 18	13 47 [illegible]	[illegible]	48,5	·98	63·0	62·4	1·790	—0,30
			23 9 [illegible]	[illegible]	71,5	·03	62·8	62·4	3·010	
180	C	May 19	6 43 [illegible]	[illegible]	57,0	·94	61·3	61·3	29·810	—0,30
			9 15 [illegible]	[illegible]	30,0	·08	61·8	61·8	29·780	
181	a	May 21	2 23 [illegible]	[illegible]	49,5	1·02	64·4	64·1	1·220	0,00
			10 46 [illegible]	[illegible]	39,5	0·08	62·8	62·4	1·820	
182	a	May 22	10 50 [illegible]	[illegible]	60,0	1·01	62·8	62·4	1·820	0,00
			16 12 [illegible]	[illegible]	63,5	0·19	63·1	62·7	1·980	
183	a	May 22	19 37 [illegible]	[illegible]	55,5	0·99	64·0	64·0	29·982	—0,17
			22 26 [illegible]	[illegible]	51,0	0·08	64·5	64·5	29·980	
184	a	May 23	11 46 [illegible]	[illegible]	41,5	1·00	62·1	62·3	29·912	—0,17
			14 35 [illegible]	[illegible]	64,0	0·06	62·5	62·5	29·894	
185	c	May 23	4 19 [illegible]	[illegible]	41,5	1·02	66·0	65·6	1·530	+0,18
			12 55 [illegible]	[illegible]	26,0	0·06	64·8	64·4	1·830	
186	c	May 24	12 59 [illegible]	[illegible]	42,0	1·00	64·8	64·4	1·830	+0,18
			19 5 [illegible]	[illegible]	9,5	0·13	66·1	65·4	2·070	
187	c	May 24	22 41 [illegible]	[illegible]	4,5	0·99	67·9	68·1	29·810	+0,18
			1 2 [illegible]	[illegible]	48,5	0·06	67·5	67·6	29·832	
188	c	May 24	1 5 [illegible]	[illegible]	37,0	1·01	67·5	67·6	29·832	+0,18
			3 18 [illegible]	[illegible]	61,5	0·08	67·0	67·1	29·864	

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers.		Barometer.	Rate.
							Upper.	Lower.		
			h m s	s	s	°	°	°		s
No. 39. Mercurial, on spring.	189	March 28	23 21 [illegible]	[illegible]	67,5	0·77	48·7	48·5	30·124	+0,39
			2 36 [illegible]	[illegible]	49,5	·21	48·6	48·3	30·074	
	190	March 28	3 37 [illegible]	[illegible]	27,5	·77	48·4	48·4	0·830	+0,39
			11 27 [illegible]	[illegible]	63,0	·18	47·2	46·8	0·980	
	191	March 28	11 31 [illegible]	[illegible]	52,5	·77	47·4	47·0	0·980	+0,39
			19 20 [illegible]	[illegible]	76,0	·17	44·7	44·0	1·100	
	192	March 29	20 2 [illegible]	[illegible]	71,5	·77	45·6	45·3	30·018	+0,32
			23 32 [illegible]	[illegible]	42,0	·21	45·0	44·5	29·954	
	193	March 29	1 11 [illegible]	[illegible]	35,5	·77	45·5	45·3	29·966	+0,32
			5 28 [illegible]	[illegible]	74,5	·16	46·3	46·2	29·958	
	194	March 29	6 9 [illegible]	[illegible]	50,5	·77	45·6	45·5	2·620	+0,32
			11 58 [illegible]	[illegible]	48,0	·20	45·5	45·1	2·780	
	195	March 29	12 4 [illegible]	[illegible]	52,5	·72	45·7	46·0	2·780	+0,32
			20 8 [illegible]	[illegible]	85,5	·14	43·9	43·3	2·930	
	196	March 30	20 52 [illegible]	[illegible]	50,5	·71	45·0	44·7	30·014	+0,26
			0 52 [illegible]	[illegible]	82,0	·16	46·0	45·9	29·968	

TABLE I.—Continued.

Pendulum.	No.	1832.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Upper.	Thermometers. Lower.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
No. 41. Lead bob, flat wood rod.	197	May 14	20 18 $\frac{21}{22}$ 23 54 $\frac{8}{19}$	$\frac{25}{26}$ $\frac{34}{45}$	23,5 26,5	0·86 ·23	50·4 50·7	50·0 50·7	29·828 29·812	—0,49
	198	May 14	2 6 $\frac{35}{36}$ 12 35 $\frac{46}{47}$	$\frac{39}{40}$ $\frac{51}{52}$	37,5 49,0	·86 ·19	50·6 51·5	50·8 51·5	1·160 1·830	—0,49
	199	May 14	12 37 $\frac{55}{56}$ 20 41 $\frac{64}{65}$	$\frac{57}{60}$ $\frac{80}{81}$	57,0 72,5	·85 ·25	51·6 50·5	51·5 50·1	1·830 2·390	—0,49
	200	May 15	21 12 $\frac{25}{26}$ 0 56 $\frac{60}{61}$	$\frac{27}{28}$ $\frac{80}{81}$	26,5 70,5	·87 ·21	51·5 51·2	51·5 51·2	29·814 29·806	—0,49
No. 40. Lead bob, round wood rod.	201	May 16	19 43 $\frac{15}{16}$ 23 30 $\frac{40}{45}$	$\frac{18}{19}$ $\frac{60}{63}$	17,0 52,0	·81 ·19	49·8 50·6	49·5 50·6	29·890 29·864	—0,49
	202	May 16	1 3 $\frac{45}{46}$ 12 14 $\frac{40}{45}$	$\frac{47}{48}$ $\frac{66}{69}$	46,5 55,0	·90 ·17	50·6 53·0	50·7 52·7	1·060 1·760	—0,49
	203	May 16	12 16 $\frac{31}{33}$ 19 30 $\frac{32}{35}$	$\frac{34}{36}$ $\frac{54}{57}$	33,5 44,5	·77 ·22	53·0 51·6	52·7 51·0	1·760 2·240	—0,49
	204	May 17	20 26 $\frac{11}{12}$ 0 13 $\frac{9}{21}$	$\frac{15}{16}$ $\frac{40}{50}$	13,5 30,0	·78 ·18	52·3 52·0	52·1 51·7	29·882 29·864	—0,49

No. 30. Iron bar. (See page 467.)

Knife edge.	No.	1831.	Disappearance.	Reappearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
B	205	Nov. 6	1 37 $\frac{21}{24}$ 2 7 $\frac{33}{34}$	$\frac{28}{31}$ $\frac{35}{36}$	26,0 34,5	0·96 0·77	46·0 46·6	45·7 46·0	29·482 29·444	+0,40
b	206	Nov. 9	20 5 $\frac{18}{19}$ 21 51 $\frac{7}{17}$	$\frac{22}{23}$ $\frac{27}{33}$	20,5 21,0	1·00 0·38	46·5 46·5	46·4 46·4	30·164 30·172	+0,30
B	207	Nov. 9	21 57 $\frac{39}{40}$ 22 57 $\frac{53}{58}$	$\frac{42}{43}$ $\frac{57}{60}$	41,0 57,0	1·02 0·60	46·8 46·5	46·4 46·3	30·172 30·210	+0,30
b	208	Nov. 9	23 6 $\frac{32}{37}$ 0 23 $\frac{3}{9}$	$\frac{34}{39}$ $\frac{15}{19}$	35,5 11,5	1·06 0·54	46·8 46·6	46·4 46·4	30·214 30·216	+0,30
B	209	Nov. 9	0 43 $\frac{8}{11}$ 2 13 $\frac{27}{46}$	$\frac{13}{16}$ $\frac{47}{48}$	12,0 42,0	1·02 0·46	47·5 47·0	46·4 46·7	30·224 30·238	+0,30
b	210	Nov. 9	2 23 $\frac{0}{1}$ 3 38 $\frac{49}{56}$	$\frac{4}{7}$ $\frac{60}{61}$	3,0 56,5	1·06 0·53	47·2 47·3	46·6 46·8	30·238 30·270	+0,30
b	211	Nov. 10	22 46 $\frac{51}{54}$ 23 1 $\frac{55}{64}$	$\frac{57}{54}$ $\frac{67}{76}$	54,0 65,5	1·02 0·91	45·0 45·0	44·4 44·4	30·434 30·434	+0,26
b	212	Nov. 10	23 8 $\frac{28}{29}$ 23 54 $\frac{19}{33}$	$\frac{31}{32}$ $\frac{31}{33}$	30,0 29,0	1·05 0·67	45·0 44·7	44·4 44·3	30·434 30·414	+0,26
B	213	Nov. 10	0 7 $\frac{41}{47}$ 1 8 $\frac{14}{21}$	$\frac{48}{54}$ $\frac{27}{34}$	47,5 24,0	1·02 0·59	45·3 44·6	44·5 44·4	30·409 30·394	+0,26
b	214	Nov. 10	1 15 $\frac{6}{15}$ 2 16 $\frac{12}{36}$	$\frac{21}{32}$ $\frac{38}{48}$	18,5 33,5	1·04 0·58	44·6 44·6	44·4 44·4	30·394 30·370	+0,26
B	215	Nov. 10	2 23 $\frac{18}{19}$ 3 23 $\frac{44}{47}$	$\frac{27}{28}$ $\frac{56}{59}$	23,0 51,5	1·02 0·58	44·8 44·6	44·4 44·4	30·370 30·354	+0,26
B	216	Nov. 13	21 4 $\frac{34}{34}$ 22 34 $\frac{19}{36}$	$\frac{38}{40}$ $\frac{33}{34}$	36,5 30,5	1·02 0·46	51·5 51·6	51·3 51·3	30·014 30·174	+0,14
b	217	Nov. 13	22 41 $\frac{34}{63}$ 0 42 $\frac{37}{65}$	$\frac{48}{77}$ $\frac{62}{72}$	55,5 59,0	1·03 0·32	51·6 51·3	51·3 51·0	30·174 30·006	+0,14

TABLE I.—Continued.

No. 30 Iron bar (continued).

Knife edge.	No.	1831.	Disappearance.	Re-appearance.	Coincidence.	Arc.	Thermometers. Outside.	Thermometers. Inside.	Barometer.	Rate.
			h m s	s	s	°	°	°		s
B	218	Nov. 13	0 48 [illegible]	[illegible]	45,5	1·03	51·3	51·0	30·006	+0,14
			1 48 [illegible]	[illegible]	48,0	0·61	51·1	51·0	29·994	
B	219	Nov. 21	1 11 [illegible]	[illegible]	13,5	1·00	44·8	43·7	29·758	+0,47
			2 11 [illegible]	[illegible]	42,5	0·58	45·0	44·0	29·770	
B	220	Nov. 21	2 15 [illegible]	[illegible]	58,5	1·00	45·1	44·1	29·770	+0,47
			3 31 [illegible]	[illegible]	33,5	0·48	45·4	44·5	29·806	
B	221	Nov. 22	20 25 [illegible]	[illegible]	15,5	·96	49·3	48·5	29·864	+0,54
			21 10 [illegible]	[illegible]	19,0	·67	49·8	49·0	29·878	
b	222	Nov. 22	21 17 [illegible]	[illegible]	15,0	·98	50·1	49·0	29·878	+0,54
			22 32 [illegible]	[illegible]	68,0	·48	50·1	49·4	29·894	
b	223	Nov. 22	22 37 [illegible]	[illegible]	56,5	·99	50·1	49·4	29·894	+0,54
			0 8 [illegible]	[illegible]	61,5	·45	50·4	49·7	29·892	
b	224	Nov. 22	0 11 [illegible]	[illegible]	44,0	1·02	50·4	49·7	29·892	+0,54
			1 27 [illegible]	[illegible]	33,0	0·48	50·7	50·0	29·904	
b	225	Nov. 23	20 1 [illegible]	[illegible]	24,0	1·02	53·2	52·6	29·990	+0,54
			21 16 [illegible]	[illegible]	65,5	0·50	53·3	52·8	29·990	
b	226	Nov. 23	21 21 [illegible]	[illegible]	29,5	·98	53·3	52·8	29·990	+0,54
			22 37 [illegible]	[illegible]	15,5	·50	53·5	53·0	29·996	
B	227	Nov. 23	22 41 [illegible]	[illegible]	35,0	1·02	53·5	53·0	29·996	+0,54
			0 11 [illegible]	[illegible]	16,0	0·46	53·9	53·3	29·984	
B	228	Nov. 23	0 13 [illegible]	[illegible]	50,5	·99	53·9	53·3	29·984	+0,54
			1 28 [illegible]	[illegible]	29,5	·50	54·0	53·5	29·984	
		1832.					* Upper.	* Lower.		
B	229	Feb. 3	4 30 [illegible]	[illegible]	21,5	·80	43·0	43·1	29·754	—0,01
			6 17 [illegible]	[illegible]	17,0	·31	42·9	42·6	29·776	
b	230	Feb. 4	20 37 [illegible]	[illegible]	30,5	·79	43·6	43·6	29·848	—0,01
			22 40 [illegible]	[illegible]	53,0	·27	44·4	44·3	29·894	

* Both inside.

TABLE II.—*Results of the preceding Table.*

Pendulum.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for Arc.	Corrections for Therm.	Corrections for Rate.	N′ and N″. See page 407.
		h m s		s				
No. 1. Platina Sphere.	1	4 13 12,5	30	506·417	+·429	−6·914	−·160	86734·576
	2	9 43 9,5	70	499·850	·493	6·558	·180	86739·469
	3	10 24 29,5	75	499·593	·530	6·572	·200	86739·640
	4	5 29 5,7	39	506·301	·392	6·706	·250	86734·735
	5	5 3 51,0	36	506·417	·407	6·980	·300	86734·348
	6	9 1 15,5	65	499·623	·578	6·861	·350	86739·228
	7	9 50 53,0	71	499·338	·523	7·083	·400	86739·099
	8	3 39 24,0	26	506·308	·536	7·196	·450	86734·184
No. 3. Brass Sphere.	9	2 34 33,0	19	488·053	+·304	−6·988	−·500	86746·876
	10	6 48 6,0	52	470·885	·304	7·166	·500	86759·607
	11	6 40 12,5	51	470·823	·372	7·315	·500	86759·564
	12	2 1 58,0	15	487·866	·502	7·381	·450	86746·867
	13	2 18 25,5	17	488·559	·424	7·380	·430	86746·307
	14	9 9 48,0	70	471.257	·323	7·221	·410	86759·371
	15	9 33 17,0	73	471·192	·282	7·217	·400	86759·395
	16	2 10 22,0	16	488·875	·449	7·137	·400	86746·377
No. 2. Lead Sphere.	17	2 50 32,5	18	568·444	+·342	−4·766	−·060	86699·504
	18	7 12 28,0	47	552·085	·349	4·865	·060	86708·419
	19	8 34 25,0	56	551.161	·295	5.213	·030	86708·572
	20	3 46 38,5	24	566·604	·293	5·554	·000	86699·714
	21	2 59 34,0	19	567·053	·324	5·548	·000	86699·510
	22	7 3 8,5	46	551·924	·302	5·340	·000	86708·048
	23	8 16 29,5	54	551·657	·331	5·419	·000	86708·150
	24	2 59 32,0	19	566·947	·355	5·480	·000	86699·666
No. 4. Ivory Sphere.	25	0 44 7,0	6	441·166	+·254	−3·947	−·430	86787·567
	26	1 43 40,0	16	388·750	·324	3·924	·430	86840·470
	27	1 43 51,5	16	389·469	·302	5·049	·460	86838·473
	28	0 51 19,7	7	439·964	·259	4·751	·460	86787·807
	29	0 51 25,0	7	440·714	·259	4·888	·480	86786·983
	30	2 16 14,5	21	389·262	·264	4·878	·480	86838·826
	31	2 16 23,5	21	389·690	·264	4·636	·480	86838·568
	32	0 51 33,0	7	441·857	·250	4·360	·480	86786·489
No. 6. Brass Sphere.	33	2 54 53,5	18	582·972	+·571	−6·008	−·520	86690·455
	34	8 43 35,5	56	560·991	·669	6·044	·560	86702·092
	35	8 43 4,5	56	560·437	·482	6·216	·600	86701·997
	36	4 41 17,5	29	581·982	·380	6·246	·630	86690·420
	37	4 12 32,5	26	582·788	·449	6·320	·630	86690·005
	38	9 19 59,5	60	559·992	·615	6·691	·630	86701·869
	39	3 15 19,0	21	558·047	·109	7·063	·630	86702·064
	40	2 54 24,0	18	581·333	·640	7·048	·630	86690·210
No. 7. Ivory Sphere.	41	1 6 11,0	7	567·286	+·404	−7·790	−·600	86696·622
	42	2 32 49,0	19	482·579	·489	7·440	·640	86750·485
	43	2 32 55,0	19	482·895	·476	7·300	·660	86750·359
	44	1 6 22,0	7	568·857	·399	7·063	·680	86696·423
	45	1 6 33,5	7	570·500	·399	6·712	·720	86695·860
	46	2 41 17,0	20	483·850	·478	6·558	·730	86750·326
	47	2 41 33,0	20	484·650	·464	6·988	·800	86749·223
	48	1 6 29,0	7	569·857	·424	6·914	·800	86695·944

Table II.—Continued.

Pendulum.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for			N′ and N″. See page 407.
					Arc.	Therm.	Rate.	
		h m s		s				
No. 5. Lead Sphere.	49	5 20 53,0	29	663·897	+·406	−5·702	−·230	86654·756
	50	6 26 21,0	36	643·916	·663	5·830	·230	86662·961
	51	7 30 1,0	42	642·881	·614	6·178	·220	86663·005
	52	4 24 46,0	24	661·916	·451	6·460	·220	86654·831
	53	7 43 10,5	42	661·679	·279	6·490	·220	86654·723
	54	8 23 4,5	47	642·224	·618	6·504	·220	86662·959
	55	7 51 29,5	44	642·943	·550	6·305	·230	86662·779
	56	5 31 4,0	30	662·133	·375	6·572	·240	86654·538
No. 9. Ivory Sphere on Cylinder.	57	1 13 18,5	7	628·357	+·296	−5·816	+·200	86669·683
	58	2 38 14,5	18	527·472	·440	5·591	·200	86722·649
	59	2 56 3,0	20	528·150	·354	5·132	·200	86722·602
	60	1 13 20,0	7	628·571	·306	4·567	·200	86671·208
No. 8. Lead Sphere on Cylinder.	61	3 13 45,5	24	484·396	+·401	−3·965	+·230	86753·400
	62	5 15 56,5	40	473·912	·632	3·594	·230	86761·893
	63	6 50 3,5	52	473·144	·608	3·454	·230	86762·601
	64	3 12 56,5	24	482·354	·408	4·054	·200	86754·797
No. 10. Brass Cylinder, with iron wire.	65	4 7 38,0	23	646·000	+·507	−4·336	+·600	86664·263
	66	8 56 58,5	52	619·587	·688	3·995	·600	86676·189
	67	8 15 37,0	48	619·521	·660	3·876	·570	86676·280
	68	3 25 1,5	19	647·447	·574	3·905	·570	86664·134
No. 11. Brass Cylinder, with Brass Rod.	69	2 46 37,0	13	769·000	+·419	−3·189	+·500	86622·450
	70	3 51 56,0	19	732·421	·655	2·858	·500	86634·217
	71	4 4 35,5	20	733·775	·634	2·556	·500	86634·058
	72	2 21 31,2	11	771·927	·451	2·522	·500	86622·289
No. 12. Brass Cylinder, with Brass Rod.	73	1 59 48,0	9	798·666	+·553	−3·793	+·520	86613·640
	74	4 37 24,5	22	756·568	·740	3·707	·520	86625·933
	75	4 25 25,0	21	758·333	·728	3·439	·520	86625·669
	76	1 46 46,0	8	800·750	·632	3·384	·520	86613·568
No. 13. Brass Cylinder, with Brass Rod.	77	4 5 0,0	17	864·706	+·291	−4·051	+·600	86596·677
	78	10 14 58,5	45	819·967	·462	3·289	·600	86608·533
	79	8 12 27,0	36	820·750	·524	3·116	·600	86608·548
	80	2 24 55,5	10	869·550	·624	3·254	·600	86596·694
No. 18. Hollow Brass Cylinder, both ends closed.	81	1 17 6,0	8	578·250	+·574	−5·012	+·490	86694·885
	82	4 7 20,5	29	511·741	·655	5·028	·490	86733·788
	83	3 58 22,0	28	510·786	·503	5·459	·450	86733·797
	84	1 55 37,0	12	578·083	·462	5·331	·450	86694·500
No. 15. Hollow Brass Cylinder, both ends open.	85	1 32 52,0	12	464·333	+·442	−6·029	+·580	86767·140
	86	3 8 39,0	27	419·222	·584	6·014	·580	86807·342
	87	3 8 54,5	27	419·796	·714	5·845	·580	86807·078
	88	1 25 11,0	11	464·636	·410	5·696	·580	86767·199
No. 16. Hollow Brass Cylinder, top open, bottom closed.	89	1 58 39,0	11	647·182	+·692	−5·058	+·660	86663·298
	90	4 6 45,5	26	569·442	·578	4·493	·660	86700·200
	91	3 47 50,0	24	569·583	·641	4·425	·660	86700·256
	92	1 58 48,5	11	648·045	·416	4·479	·660	86663·246
No. 17. Hollow Brass Cylinder, top closed, bottom open.	93	1 40 36,5	14	431·178	+·402	−5·102	+·430	86796·493
	94	3 2 57,0	28	392·036	·746	4·930	·430	86837·026
	95	3 29 34,5	32	392·953	·694	4·434	·540	86836·540
	96	1 33 33,5	13	431·808	·565	4·202	·540	86797·081

TABLE II.—Continued.

Pendulum.		No.	Total Interval.			No. of Coin-cid.	Mean Interval.	Corrections for			N' and N''. See page 407.
								Arc.	Therm.	Rate.	
			h	m	s		s				
No. 14. Solid Lead Cylinder.		97	7	45	43,0	34	821·853	+·250	−2·079	+·500	86608·911
		98	7	38	37,5	35	786·214	·610	1·820	·600	86619·150
		99	6	21	17,0	29	788·862	·263	1·301	·600	86618·602
		100	8	2	13,5	35	826·671	·234	1·559	·600	86608·295
No. 19. Hollow Brass Cylinder hermetically sealed.		101	1	12	41,0	9	484·555	+·536	−4·167	+·200	86753·184
		102	3	5	23,0	26	427·808	·681	3·870	·200	86800·931
		103	3	12	26,0	27	427·629	·685	3·721	·300	86801·364
		104	1	29	14,0	11	486·727	·501	3·460	·300	86752·366
No. 20. Lead Lens.		105	4	6	19,0	22	671·773	+·383	−3·252	+·520	86654·881
		106	9	3	57,0	50	652·740	·476	3·341	·520	86662·386
		107	10	41	47,5	59	652·670	·331	3·588	·440	86661·942
		108	4	5	12,5	22	668·750	·383	4·396	·440	86654·820
No. 21. Copper Cylindrical Rod.		109	1	51	45,5	32	209·547	+·230	−4·327	The clock making 86045^{s}·291 in a day.	85219·963
		110	3	23	22,0	57	214·070	·432	4·449		85237·383
		111	3	1	55,0	51	214·019	·425	4·376		85237·209
		112	2	2	15,0	35	209·571	·239	4·294		85220·085
No. 25—26. Brass bar, ¼ inch thick.	Knife edge.										
	A	113	8	4	22,5	32	908·203	+·548	+3·577	+·230	86214·089
	A	114	3	33	57,5	15	855·833	·396	3·910	·230	86202·627
	B	115	2	52	29,5	12	862·458	·522	2·822	·200	86203·186
	B	116	4	49	55,0	19	915·526	·903	3·362	·200	86215·720
	B	117	7	51	59,5	31	913·532	·659	3·997	·200	86215·700
	B	118	3	6	14,2	13	859·578	·462	3·577	·150	86203·150
	A	119	19	42	59,5	77	921·812	·221	1·164	−·180	86213·748
	A	120	4	20	48,0	18	869·333	·305	1·012	·180	86202·363
	A	121	4	5	55,0	17	867·941	·321	+1·314	·180	86202·362
	a	122	1	0	3,0	4	900·750	·580	−5·689	+·430	86203·480
	a	123	2	0	4,5	8	900·562	·641	5·765	·450	86203·446
	a	124	10	24	1,5	39	960·038	·449	5·754	·500	86215·202
	a	125	2	15	8,0	9	900·889	·548	5·832	·520	86203·425
	b	126	1	14	33,0	5	894·600	·807	5·172	·630	86203·106
	b	127	2	58	57,5	12	894·792	·469	5·043	·700	86203 008
	b	128	14	3	45,0	53	955·189	·357	4·904	·700	86215·147
	b	129	2	58	37,0	12	893·167	·464	4·797	·700	86202·898
	b	130	3	13	26,5	13	892·808	·442	4·646	·700	86202·949
	B	131	1	58	29,5	8	888·687	·613	3·858	·700	86203·010
	B	132	2	13	15,5	9	888·389	·586	3·849	·700	86202·927
	B	133	18	44	22,5	71	950·176	·234	4·030	·700	86215·043
	B	134	3	27	55,5	14	891·107	·408	3·987	·710	86203·215
	B	135	3	27	58,5	14	891·321	·400	4·051	·720	86203·200
	A	136	3	12	42,5	13	889·423	·416	3·944	·700	86202·889
	A	137	1	43	33,5	7	887·642	·700	3·965	·700	86202·762
	A	138	2	42	55,5	11	888·682	·469	3·892	·660	86202·791
	A	139	13	23	52,0	51	945·725	·374	3·965	·640	86214·332
	A	140	2	28	12,5	10	889·250	·477	4·074	·620	86202·702
	A	141	2	28	17,0	10	889·700	·477	4·074	·580	86202·760
No. 31—34.	A	142	2	53	53,5	11	948·500	+·533	−7·823	+·150	86210·677
	A	143	14	17	18,0	50	1028·760	·354	8·922	·150	86223·613
	A	144	3	11	11,0	12	955·925	·522	9·288	·150	36210·624
	B	145	1	42	58,5	7	882·642	·838	9·051	·150	86196·160
	B	146	9	30	9,5	36	950·263	·678	9·568	·150	86209·415
	B	147	2	42	35,0	11	886·818	·616	9·718	·100	86196·144
	D	148	13	4	8,0	49	960·163	·342	10·495	·100	86209·977

TABLE II.—Continued.

	Knife edge.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for			N′ and N″. See page 407.
						Arc.	Therm.	Rate.	
			h m s		s				
Brass bar, ¾ inch thick.	D	149	2 43 44,5	11	893·136	+·587	−10·580	+·000	86196·531
	D	150	5 43 8,0	23	895·130	·331	10·539	·200	86196·947
	C	151	2 40 20,5	10	962·050	·669	9·934	·200	86211·318
	C	152	13 51 15,5	48	1039·073	·434	9·955	·200	86224·377
	C	153	3 11 19,5	12	956·625	·581	8·749	·300	86211·497
	A	154	3 24 1,0	13	941·615	·358	6·099	·400	86211·144
	A	155	18 53 52,0	67	1015·403	·324	6·392	·400	86224·153
	A	156	3 8 35,0	12	942·917	·291	6·694	·400	86210·736
	B	157	2 55 0,5	12	875·416	·660	6·703	·450	86197·015
	B	158	3 9 33,0	13	874·846	·612	6·711	·450	86196·830
	B	159	14 1 56,0	54	935·482	·429	6·293	·450	86209·868
	B	160	2 54 23,0	12	871·917	·590	6·220	·450	86196·636
	D	161	4 23 33,0	18	878·500	·388	6·973	·400	86197·116
	D	162	18 37 39,5	71	944·500	·275	7·297	·400	86210·423
	D	163	2 55 59,0	12	879·916	·586	7·620	·400	86196·983
	C	164	2 54 3,0	11	949·364	·607	7·499	·400	86211·491
	C	165	13 41 30,5	48	1026·885	·443	7·499	·400	86225·068
	C	166	3 10 5,0	12	950·416	·428	7·348	·400	86211·665
No. 35—38. Brass tube.	Plane.								
	c	167	2 13 0,5	14	570·036	+·918	−7·065	−·000	90080·030
	c	168	0 58 55,0	7	505·000	·974	6·948	·000	90039·355
	A	169	1 49 47,5	13	506·731	·375	5·940	·000	90040·986
	A	170	1 35 6,5	10	570·650	1·082	5·445	·000	90082·155
	C	171	2 12 55,0	16	498·437	·357	4·163	·000	90036·808
	C	172	3 34 31,0	23	559·609	·803	3·870	·000	90077·201
	a	173	3 17 3,0	21	563·000	·807	5·085	·000	90077·935
	a	174	1 23 20,0	10	500·000	·576	4·950	·000	90037·374
	A	175	3 27 56,5	22	567·114	·648	−0·045	·313	90084·833
	A	176	5 48 58,5	37	565·905	·393	+0·135	·313	90084·076
	A	177	2 6 19,0	15	505·267	·353	−0·630	·322	90044·918
	A	178	2 31 27,5	18	504·861	·257	−0·293	·322	90044·872
	C	179	9 22 23,0	60	562·383	·236	+0·540	·313	90082·324
	C	180	2 31 33,0	18	505·167	·305	−0·203	·313	90045·236
	a	181	8 22 50,0	53	569·245	·346	+0·922	·000	90087·004
	a	182	5 22 3,5	34	568·338	·505	0·607	·000	90086·341
	a	183	2 48 55,5	20	506·775	·330	1·012	·177	90047·747
	a	184	2 49 22,5	20	508·125	·302	0·180	−·177	90047·835
	c	185	8 35 44,5	55	562·627	·312	1·710	+·187	90084·209
	c	186	6 5 27,5	39	562·244	·412	1·665	·187	90084·045
	c	187	2 21 44,0	17	500·235	·297	2·632	·187	90045·034
	c	188	2 13 24,5	16	500·281	·341	2·407	·187	90044·886
No. 39. Mercurial Pendulum.		189	3 14 42,0	12	973·500	+·344	No correction required.	+·390	86578·238
		190	7 50 35,5	31	910·822	·315		·390	86590·424
		191	7 49 23,5	31	908·500	·304		·390	86590·898
		192	3 29 30,5	13	966·961	·344		·320	86579·368
		193	4 17 39,0	16	966·187	·295		·320	86579·462
		194	5 48 57,5	23	910·326	·335		·320	86590·477
		195	8 4 33,0	32	908·531	·245		·320	86590·762
		196	4 0 31,5	15	962·100	·260		·260	86580·127
No. 41. Lead Cylinder with flat Rod.		197	3 36 3,0	25	518·520	+·428	No correction required.	−·490	86066·681
		198	10 29 11,5	70	539·307	·383		·490	86079·423
		199	8 5 15,5	54	539·176	·443		·490	86079·473
		200	3 44 44,0	26	518·615	·412		·490	86066·726
No. 40. Lead Cylinder with round Rod.		201	3 47 35,0	25	546·200	+·351	No correction required.	−·490	86083·481
		202	11 11 8,5	71	567·162	·384		·490	86095·220
		203	7 14 11,0	46	566·326	·355		·490	86094·740
		204	3 47 16,5	25	545·460	·322		·490	86083·032

TABLE II.—Continued.

No. 30. Iron bar.

Knife edge.	No.	Total Interval.	No. of Coincid.	Mean Interval.	Corrections for				True number of vibrations in a mean solar day.
					Arc.	Therm.	Barom.	Rate.	
		h m s		s					
B	205	0 30 8,5	2	904·250	+1·224	−4·637	+14·301	+·400	86220·190
b	206	1 46 0,5	7	908·643	·726	4·478	14·625	·300	86220·999
B	207	1 0 16,0	4	904·000	1·052	4·493	14·638	·300	86220·346
b	208	1 16 36,0	5	919·200	1·016	4·478	14·651	·300	86223·499
B	209	1 30 30,0	6	905 000	·856	4·435	14·651	·300	86220·433
b	210	1 15 53,5	5	910·700	·997	4·392	14·657	·300	86221·818
b	211	0 15 11,5	1	911·500	1·527	5·054	14·821	·260	86221·976
b	212	0 45 59,0	3	919·667	1·194	5·069	14·818	·260	86223·309
B	213	1 0 36,5	4	909·125	1·052	5·040	14·803	·260	86221·002
b	214	1 1 15,0	4	918·750	1·048	5·054	14·795	·260	86222·967
B	215	1 0 28,5	4	907·125	1·024	5·054	14·786	·260	86220·524
B	216	1 29 54,0	6	899·000	·856	3·085	14·432	·140	86220·129
b	217	2 1 3,5	8	907·937	·670	3·128	14·430	·140	86221·791
B	218	1 0 2,5	4	900·625	1·077	3·172	14·396	·140	86220·574
B	219	1 0 29,0	4	907·250	1·000	5·213	14·511	·470	86220·302
B	220	1 15 35,0	5	907·000	·860	5·073	14·508	·470	86220·247
B	221	0 45 3,5	3	901·167	1·081	3·802	14·405	·540	86220·473
b	222	1 15 53,0	5	910·600	·837	3·672	14·398	·540	86222·338
b	223	1 31 5,0	6	910·833	·807	3·571	14·391	·540	86222·450
b	224	1 15 49,0	5	909·800	·881	3·485	14·397	·540	86222·401
b	225	1 15 41,5	5	908·300	·912	2·664	14·338	·540	86222·881
b	226	1 15 46,0	5	909·200	·868	2·607	14·338	·540	86223·077
B	227	1 29 41,0	6	896·333	·854	2·534	14·323	·540	86220·504
B	228	1 14 39,0	5	895·800	·872	2·462	14·313	·540	86220·362
B	229	1 46 55,5	7	916·500	·473	5·501	14·544	−·010	86220·962
b	230	2 3 22,5	8	925·312	·423	5·193	14·561	·010	86223·033

Note to page 417.—It was omitted to be stated, with reference to the pendulum No. 35—38, that the steel collars, which are attached to the tube, and on which the pendulum swings, are divided, on their outer circumference, into 16 equal parts; thus making 8 several diameters (numbered from 1 to 8) on which the vibrations of the pendulum may be varied. I have also got 3 separate pairs of agate knife edges, differing from each other in sharpness, for the purpose of ascertaining whether the results are affected by such an alteration of this part of the apparatus. But, at present, I have not made any experiments with this view.

XX. *An Account of the Magnetical Experiments made on the western coast of Africa,* 1830-1, *by Commander* Edward Belcher, *of H. M. S. Ætna. Communicated by the Rev.* George Fisher, *through Captain* Beaufort, *R.N. F.R.S.*

Read June 21, 1832.

I HAVE the honour of communicating the results of some experiments made on the western coast of Africa, by Commander Edward Belcher, of His Majesty's ship Ætna, for the purpose of determining the relative horizontal intensities of the magnetic force, on the different parts of the coasts he has been lately surveying.

The experiments were made with four needles, constructed for the purpose, on nearly the same plan as that adopted by Professor Hansteen, and made by Mr. Dollond. They were nearly cylindrical, and furnished with a moveable collar or suspension stirrup, for the purpose of adjusting them horizontally, and were respectively four, three and a half, three, and two and a half inches in length.

Captain Belcher has kindly sent me his observations with these needles, which he has most accurately made, together with similar ones made in England since his return; and the near agreement between these and others made by myself before their embarkation, affords a satisfactory proof that the magnetism of the needles has not undergone any material change during the period of the voyage; a proof most essential in obtaining a correct result in experiments of this nature, and the want of which has rendered many others made of late years in different parts of the world, little better than useless.

The sudden changes in the intensities of magnetic needles, particularly those kept on ship-board, arising from a variety of causes, are well known to those accustomed to use them. Hence arises the necessity in experiments, such as those described in this paper, of frequently repeating them at the same place, between the same limits of arc, since the value of the result depends upon the permanency of the magnetism in the needles, in order that the experiments made with them may be strictly comparative.

The observations were frequently repeated in different places, at some distance from each other, at each station, and a mean of these taken as one result, as at Goree and Rio Nuñez. The observations at Bathurst were made in the Government-house, and also at some distance from it, but exhibit no material difference.

By thus varying the place of observation at each station, a mean result is obtained, which is most probably more free from errors, particularly those arising from irregularities caused by the vicinity of iron ores and other peculiarities of the soil; an instance of which took place at the Isles de Los. Captain BELCHER observes, that " these islands being of volcanic origin, the sands even contain iron sufficient to influence the needle, and the rocks in some positions so forcibly, as to cause one of the needles suspended horizontally, to cease almost instantaneously after twenty vibrations."

The whole detail of Captain BELCHER's experiments is very extensive; the following tables therefore contain only the abstract of them, together with the results which I have deduced from them. They do him the greatest credit, and evince his indefatigable exertions, as well as excellent judgement.

Table I. contains the means of a great number of observations obtained by observing the times of completing a certain number of vibrations with the respective needles vibrating between the same limits of arc, viz. 30° and 10°.

Table II. contains the horizontal forces at the different places, considering the horizontal force at Portsmouth equal to unity, and computed from the formula φ varies as $\frac{n^2}{t^2}$, where t is the time of completing n vibrations of the needle, when solicited by the force φ. The needles were suspended by a few fibres of silk.

Table I.

Date.	Place.	Needle No. 1.	Needle No. 2.	Needle No. 3.	Needle No. 4.	Fahr. Therm.	Remarks.
		m s	m s	m s	m s	°	
1830, October	Portsmouth.	4 57·45	4 20·8	4 30·2	3 37·6	66	100 Vib^{ns} before sailing.
1831, August	Portsmouth.	4 58·44	4 20·05	4 32·86	3 37·22	70	100 Vib^{ns} after voyage.
1830, December 1..	Bay of Hann.	4 7·7	3 41·0	3 50·0	3 2·7	78	110 Vib^{ns} after voyage.
——— 3..	Bay of Dacar.	4 11·0	3 40·3	3 50·1	3 3·3	78	110 Vibrations.
——— 4..	Bay of Dacar.	4 6·8	3 36·33	3 50·66	3 4·13	78	110 Vibrations.
1831, Mar. and Ap.	Rio Nuñez..	4 8·22	3 37·06	3 47·54	3 1·55	82	110 Vibrations.
May	Bathurst....	4 10·88	3 38·7	3 49·55	3 3·25	77	110 Vibrations.
July	Cape Blanco.	3 59·43	3 28·91	3 39·43	2 55·53	64	100 Vibrations.

Table II.

Place.	Latitude North.	Longitude West.	Horizontal Force.	Thermometer.
	° ′	° ′		°
Portsmouth	50 48	1 6	1·0000	68
Cape Blanco	20 47	17 4	1·5423	64
Goree	14 40	17 25	1·7081	78
Bathurst (river Gambia) ..	13 8	16 33	1·7050	77
Rio Nuñez..............	10 36	14 42	1·7362	82

November, 1831.

XXI. *Observations on the Anatomy and Habits of Marine Testaceous Mollusca, illustrative of their mode of feeding.* *By* Edward Osler, *Esq.* *Communicated by* L. W. Dillwyn, *Esq. F.R.S.*

Read June 21, 1832.

In studying the Mollusca, we shall probably obtain more satisfactory results by tracing the organization connected with each important function through different classes of the animals, than by complete dissections of individual species. The data afforded by the first mode of investigation are more easily and effectually applied in future researches; and as they necessarily connect the study of function with that of structure, they enable the zoologist to infer with tolerable certainty those habits, which the pelagic character of the animal, or his inability to procure living specimens, prevent him from observing.

Thus examining the mollusca in detail, we shall find no part of their organization so interesting as that by which they take their food. Affording a general basis for scientific arrangement in the higher departments of zoology, it must be a still more certain key to the habits and general structure of those lower classes of animals in which the greater part of the organs are directly connected with this function.

We ought not to be surprised that so little has hitherto been done to elucidate the subject. The dissection is very difficult, from the small size and great softness of the parts; and its results are often deceptive, for it is not always easy to determine whether any particular appearance be natural, or caused by the knife. The microscope affords very little assistance in distinguishing the parts already dissected, and would be productive only of embarrassment in the attempt to display them.

Cuvier, in his work on the Mollusca, leaves this part of his subject nearly untouched. His allusions to it are for the most part but vague generalizations; and where he enters into detail, as in the trunk of *Buccinum undatum*, he falls continually into error.

In the descriptions I have to offer, and in the drawings by which they are illustrated, I propose to guide the naturalist through the successive stages of a dissection which may enable him, without much difficulty, to display the parts for himself. Where the subjects are so very small, no care will always prevent oversights, and even errors; and after every precaution to ensure accuracy, it is probable that I may be corrected in some points by naturalists who enjoy opportunities for dissecting recent specimens of the larger tropical mollusca. I wish therefore to show how the different parts may be displayed without injury; or at least, by what mode of dissection I have arrived at my own conclusions.

The herbivorous mollusca which I have examined have three distinct modes of feeding. They browse with opposite horizontal jaws—they rasp their food with an armed tongue, stretched over an elastic and moveable support—or they gorge it entire. *Trochus crassus* is a convenient example of the first; *Turbo littoreus* of the second; and *Patella vulgata* of the third.

Trochus crassus is furnished with a pair of cartilaginous jaws, whose superior margins are thickened and rounded; and which are so united by a ligament along their inferior edges, that they open and close like a book. A small accessory cartilage is loosely connected by a ligament to the posterior extremity of each. Between the jaws, and extending about half an inch beyond them, is the tongue, not flat, as in Turbo, and Patella, but folded into a semi-cylinder, and whose margins are furnished with a membrane which is expanded over the rounded upper edges of the jaws. The tongue is armed on either side with a series of imbricated and lamellar teeth, waved like an Italic *ʃ*, set in a direction obliquely forward and downward, and whose serrated edges incline backward. The space between these opposite series, which forms about two fifths of the breadth of the tongue, is set with corresponding transverse rows of small sharp teeth, whose points have a direction similar to that of the lamellar ones. The tongue, thus arming the opposite jaws, is secured in its place by the lingual membrane, and by the muscles inserted into it.

The movements of the jaws and tongue are effected by three sets of muscles. The first of these (Plate XIV. *b*, fig. 2 and 3.) occupy all the face of the jaw, and are inserted along the lingual membrane, and around the inferior half of the mouth. Their lowermost fibres on either side, inserted into the extremity of the lingual membrane, may be considered as distinct muscles, and I have figured them accordingly (*c*, fig. 3.), for they are readily distinguished in their course, and admit of being displayed separately by dissection. The action of the whole mass will project, and expand the jaws, and at the same time raise and throw forward the tongue. The jaws are closed by the transverse muscle, (*d*, fig. 2.); and the portion of food they have seized is cut away by a retraction of the tongue, effected by a third pair of muscles inserted into its lower part, (*e*, fig. 2 and 3.), and which, arising from the accessory cartilages, pass around the jaws, and run forward and upward to their insertion.

The teeth are rolled in a longitudinal direction, and to such an extent, that their inserted as well as their free edges are directed backward. Thus they form so many springs, which, yielding at first to the resistance of the food, will afterwards, by their elasticity, throw back towards the stomach the portion they have separated.

The stomach rests upon the jaws, opening directly over their active portion, without the intervention of any œsophagus. A pair of triangular lips project from its opening, and dip between the jaws to receive the food. In front of these is a double semi-cartilaginous valve, resting upon the fore part of the jaw, apparently furnished with some minute muscles, and which appears calculated, not merely to prevent the escape of any fragments of food, but also to bring them within reach of the lips. A pair of prominent parallel ridges are continued backward from the lips into the stomach for two thirds of its length, which, as they form a complete tube by closing their edges, may be considered as an internal œsophagus. I am not aware that a structure corresponding generally to this has been previously noticed.

To display all these parts, the mantle and spire are first to be removed, and the integuments of the body divided from the muscle of the spire on either side as far as the tentaculæ. The extremity of the tongue, which crosses the body from right to left, a little behind the jaws, is now to be disengaged. The detached integuments, with the viscera adhering to them, are next turned forward as far as the attachment of the stomach to the jaws; in doing which we divide a small muscle on either side, which secures the stomach. Raising the jaws by the extremity of the tongue, and dividing some delicate ligaments which connect them with the floor of the cavity, we have brought into view all the muscles already described; and the insertion of the retractors of the tongue will be seen, as in fig. 3, by dividing the ligament which unites the jaws. There are, in addition, a small muscle (*f*, fig. 3.) passing forward from the tongue to be inserted into the floor of the cavity on the right side; a pair of delicate muscles, not figured, which arise from the posterior part of the jaws, and pass forward over the lateral muscles to be inserted near the sides of the mouth; and a very small one (*g*, fig. 1. and 3.) which occupies a hollow near the point of the jaws, and assists in expanding them.

The stomach may now be turned forward, detached from the jaws, and opened longitudinally on the under part. The contained œsophagus, the lips, with a pair of very small internal lips between them, and the valves in front, will then be seen, in a favourable specimen, as in the figure.

The nervous system, which in some of the mollusca embarrasses the dissection from its size, is here very inconsiderable: indeed, it is only by a very careful examination that it can be discovered. A pair of very small ganglia at the base of the tentaculæ are connected by a cord which crosses the fore part of the stomach. A filament surrounds the attachment of the stomach to the jaws, and another runs along the left side to the back of the same organ, which it nearly crosses. The whole will be displayed by carefully detaching the integuments from above; but this is a task of extreme difficulty, for the stomach is almost of a pulpy texture, and tears with the slightest force.

In *Turbo littoreus*, the parts are far more simple than in Trochus; and the dissection, notwithstanding their very small size, is attended with fewer difficulties. The body being cleared from the spire and mantle, the integuments are to be completely cut away, as in fig. 5, leaving the contained parts in their natural situation upon the muscle of the spire. The fleshy mass connected with the mouth, which in the largest specimen scarcely exceeds the size of a hemp-seed, will then be seen in front, with the œsophagus cresting it, and running back to its termination in an elongated stomach. The extremity of the tongue, wound into a compact spiral, rests upon the stomach; while the salivary glands, a soft, yellow, granular substance, occupy the space between the spiral and the mass of the mouth. All these parts are to be distinguished through the integuments. The chief caution to be observed in this stage of the dissection is to avoid entangling the point of the knife in the spiral of the tongue.

Having carefully removed the salivary gland, we expose a very considerable nervous system. A plexus, attached on either side at the base of the tentaculæ, passes around the back of the fleshy mass, as if to secure it, being involved in such a quantity of a dark-coloured substance as to appear almost like a muscle. Two others, in which the nervous filaments are rather more distinct, are attached to the projecting ears of the œsophagus, and pass to the floor of the cavity. The whole is to be cleared away, and the parts will then appear as represented in fig. 5.

In this figure, the dark fleshy mass is seen in its natural position upon the muscle of the spire. The tongue, marked *e*, passes from under it, and runs back to its termination in the spiral *f*. The œsophagus, distinguished by its prominence and colour, is traced from the mouth, and one of its ears is seen at *i*. Under the fleshy mass are three muscles: a small one, *c*, arising from the tongue, and inserted into the middle of the floor of the cavity; another, *d*, arising by a double origin from either side of the insertion of the preceding, and running forward to blend with the sphincter of the mouth; and a pair of considerable ones, which cover the base of the fleshy mass, arising from its posterior part on either side, and terminating by a broad insertion immediately below the sphincter. The last, whose situation is indicated at *b*, are evidently the muscles upon which the act of feeding depends.

The stomach is now to be turned forward; and with very little assistance from the knife, the œsophagus will be separated from its loose attachments, exposing the internal parts, as in fig. 6. In the centre appears a considerable prominence, broad and flattened behind, and narrow on its crest and in front; along which the active portion of the tongue is stretched, and over which the lingual membrane is expanded. The back part is divided down the centre by a deep vertical groove, in which the continuation of the tongue is buried. The pharynx is furnished with a pair of strong longitudinal muscles, (*h*,) which arise from the upper part of the mouth; and the stomach when laid open, presents a series of transverse prominent ridges, which cover every part, except a deep channel extended from the œsophagus to the intestine.

The dissection will be completed by turning back the lingual membrane, and dividing a delicate expansion down the groove which the tongue had occupied. The divided membrane will immediately slip down, and expose a pair of elastic bodies, set, like acorns in their cups, in a thick base, from which this membrane is expanded to cover them. A process from the base is given off from between them, forming a loop, through which the tongue passes. The elastic bodies are narrowed in front, where they are connected by a vertical band; and they are united to the base in which they rest, only at a small point anteriorly, their larger posterior portions being free. The apparatus is represented, fig. 7, in which the elastic bodies are separated to show the membrane which connects them in front.

The tongue, a flat strap-shaped organ, is more than two inches long. It presents three longitudinal ranges of teeth, which recline backward, and are set like scales, with very little elevation of their edges. In the two outer rows, the teeth are single, irregularly crescentic in shape, and set by their convexity: in the middle one, each transverse range contains several, which are small, and nearly square. All are too minute to be distinguished, except under a high magnifying power. The magnified lingual membrane appears beautifully reticulated.

From a consideration of the whole structure, the action of the parts may be readily inferred. The contraction of the broad muscles brings the lower and posterior part of the fleshy mass forward; and the tongue, thus thrown backward with a circular motion, will act effectively upon the food which the external lips have brought within its range. It is probable that the contraction of the columnar muscles of the pharynx is synchronous with this motion, as the opening into the œsophagus would thus be advanced to receive the portion cut away. The reaction of the elastic bodies, which are necessarily compressed in effecting the stroke, will restore the parts to their former position.

Turbo littoreus feeds upon the softest algæ. I have observed it devouring a minute filament, which entered the mouth by a succession of jerks, repeated at very short intervals. In this case it is probable that the filament passes undivided into the stomach. When browsing upon larger fragments, the portions cut away are so very small that the impressions left can be seen only by a close inspection.

It is not to be questioned that *Patella vulgata* has considerable power of locomotion. I have taken one from the side of a recently stranded wreck, some feet above the beach; but it is certain that it often remains for life on the same spot. Large specimens are always to be found adhering to an irregular and naked rock, with their shells distorted in exact correspondence with all its inequalities. In such situations they necessarily depend for their food upon the fragments floating in the water; and on the rocky shores where they are found they will derive an abundant supply from this source. The little eddy which plays around them from the motion of the tides and waves, will, when the shell is raised from the rock, bring any floating fragments within reach of the lips.

Patella gorges its food entire. This fact might have been inferred from its anatomy, and it is proved by observation. Some time since, Mr. DILLWYN and myself, dissecting some specimens, found in the stomach of one of them a portion of a fucus so large, that he was enabled at once to recognise it as *F. pinnatifidus*; and in a recent communication he informs me that he has found fragments of *Ulva linza* in the stomachs of others. It is not however to be supposed that Patella never feeds upon growing marine plants: indeed I have seen it in the act of preying upon the soft dark substance so often found covering the shell of *Trochus umbilicatus*.

The jaws of Patella are furnished with a skeleton far less simple than that of Trochus. A pair of triangular cartilages, which I shall call the "lateral jaws," are united by a ligament along their base to the point, and each has a smaller "posterior cartilage" articulated with its extremity. A pair of "accessory cartilages" of the size and shape of linseed, rest, with their thicker extremities forward, on the outside of the lateral jaws. Corresponding with the centre of these, within the jaws, arise a pair of elastic and pyriform bodies, which sink between the expanded jaws, and rise above them when closed. In addition to this apparatus, there is a bony upper jaw, formed of a central portion, whose figure is the vertical section of a hollow cone, with a pair of broad processes like wings given off from its sides; and with a distinct and moderately broad margin, whose extremities are free, surrounding the base. The muscles of the pharynx arise from the superior edge of the upper jaw, and the active portion of the tongue corresponds with its concavity.

The tongue is more than four inches long. Running over the opening between the lateral jaws, it dips behind them, passes along a deep hollow in the foot nearly to the extremity of the animal, is then brought in a curve upward and forward under the investing membrane of the body, and finally, doubling on itself, returns to the posterior part of the jaws, where its extremity is attached. Like the tongues of nearly all the mollusca, it becomes soft towards the end. The teeth are strong, prominent, and erect, with the points curved backward. They are set four together in transverse rows, so compactly, that they appear as if united. The distance between these rows is equal to two thirds the breadth of the tongue; and in their intervals on either side are other rows, disposed obliquely, and with two teeth in each. The principal lingual membrane, into which the muscles are inserted, is attached along the upper part of the accessory cartilages; but there is in addition a proper one, resting upon the other, and which alone is so firmly attached to the tongue as to admit of being removed with it.

In the act of feeding, Patella opens the mouth laterally. The integuments, adhering firmly to the bony upper jaw, expand the free extremities of its margin, and separate a pair of soft lips attached within these extremities, whose opening is of course vertical. A single large and complicated muscle now closes the jaws, and retracts the tongue, whose hooked teeth draw up the food to the opening of the pharynx. Increased effect is given to this action, partly by a pair of muscles inserted into the wings of the upper jaw, which press its concavity against the teeth; and partly by the projection of the elastic pyriform bodies, which raises the tongue above the level of the lateral jaws. The parts will be restored to their original position, and the cartilages at the same time moderately expanded, by three pairs of small muscles; of which the first (*e*, fig. 13.) act upon the upper jaw; the second, (*k*, fig. 12.) upon the accessory cartilages; and the third, (*f*, fig. 11. & 12.) upon the extremity of the tongue itself.

In dissecting the parts, we begin by removing all the soft portion, comprehending the liver, ovarium, and intestines. The tongue is to be carefully disengaged, and it will be right to preserve the stomach, which is found without difficulty, on the left side, resting on the ovarium. Having divided and turned aside the integuments of the head, and thrown the stomach forward, separating the pharynx from its attachments as far as the upper jaw, we shall have exposed the parts as in fig. 10.

In this figure we observe a muscular apparatus in the pharynx similar to that of Turbo, each columnar muscle having a valvular appendage connected with it, which appears to close the opening. The active portion of the tongue is seen on the part which the pharynx had covered, and surrounding this part is an irregular edge, representing the loose membrane from which the pharynx had been separated, and which, acquiring a firmer attachment as it recedes, is at length blended with the muscles. The accessory cartilages cause the breadth of the jaws in front, and the posterior cartilages are marked by the rounded projections between which the tongue descends. None of the muscles can be distinctly displayed in this view.

Raising the whole mass, a number of small muscles are seen passing forward from the extremity of the jaws to the floor of the cavity, and forming two series; the first, inserted across the middle of the cavity; the second, which appears as a single broad muscle, a little within the mouth. The whole are to be removed, and the jaws may be detached altogether.

A pair of very thin muscles (*e*, fig. 13.) may now be traced passing from the posterior extremities over the accessory cartilages to the wings of the upper jaw, which they raise. Underneath appears a broad straight muscle, whose fibres, as it advances towards the mouth, separate to either side, exposing a yellow-looking mass, which might be mistaken for a gland, but which is an extension of the soft lips forming a considerable cavity. Within this cavity are found the extremity of the tongue, and a small conical papilla, striated transversely, which terminates the lingual membrane, and which is probably the organ of taste, as I observe it to be constantly thrown forward in the act of feeding. The cavity is to be laid open; and the muscles being divided and turned aside, the parts will appear as in fig. 11.

All the principal muscles are now exposed. Those which have been turned aside (*d*, *d*) are inserted into the wings of the upper jaw, which they depress and retract. The narrow straight muscles (*f*, *f*) arising from the extremity of the posterior cartilages, pass on beneath the cavity of the mouth to the extremity of the lingual membrane. Near the insertion of these muscles their outermost fibres separate, and pass to the attachment of the pharynx, behind which they are united, forming for it a sort of sphincter. There is a minute muscle between *f*, *f*, which, to prevent confusion, I have not figured along its whole course; it is inserted under the lingual papilla, which it retracts. The transverse muscle *g*, and the oblique ones seen between the jaws, are portions of the great retractor of the tongue.

To display this muscle, the upper jaw with its muscles, and the walls of the cavity of the mouth, are to be removed; and the muscles *f*, *f* detached, and thrown forward. Having divided and turned aside the transverse fibres, *g*; separated the ligamentous attachments which secure the lower edges of the accessory cartilages; and cut the muscles *k*, *k* which throw the cartilages forward, we may slip out the jaws, and display the muscle as in fig. 12. It is composed of three distinct portions;—the transverse fibres *g* which embrace and compress the lateral jaws; a straight column on either side *h*, attached along the under surface of the lingual membrane; and the oblique portions *i*, *i*, which, like the retractor muscles in Trochus, pass around the posterior cartilages, and run forward to be inserted into the tongue itself. The only attachment of this muscle to the jaws being at the extreme points of the posterior cartilages, it is enabled to play over them with the greatest freedom.

Chiton appears to feed like Patella, but there is considerable modification in the structure of the parts. A pair of simple lateral jaws, rather membranous than cartilaginous, constitute the whole skeleton. The tongue is projected around the point of these jaws by a pair of muscles corresponding to *f*, *f*, fig. 11. and 12; and is retracted by three pairs of powerful muscles; of which two agree with *h*, *h* and *i*, *i*, fig. 12; while the third, arising from the tendon of the second valve of the shell, is inserted into the upper part of the tongue. Between the insertions of the last pair is the opening of the pharynx. The tongue is set on either side with two rows of large teeth, of which the inner present the form of circular discs, with very blunt edges; the outer, corresponding to the interstices of the first, are prominent and falciform, with the points directed inward. The space between the inner rows is armed with ranges of smaller teeth. Under the opening of the pharynx, the tongue enters a sheath, in which its opposite edges are closely folded together. It consequently expands as it passes over the point of the jaws, and closes when retracted. Occupying the centre of the mouth is a large solid papilla, with an expanded cup-shaped extremity. It is furnished with an apparatus of muscles, and is probably a gustatory organ, like that already noticed in Patella. The extremity may, perhaps, act as a sucker, to seize the food, and convey it to the tongue.

I have observed a third modification of a structure fitted for gorging food, in a small Patella from the West Indies (*P. mammillaris*, Linn.). There is simply a very muscular mouth and pharynx; and an elastic mass very closely resembling that in *Turbo littoreus;* but neither cartilage, tongue, nor hard parts of any description.

In all the display of instinct there is perhaps nothing more extraordinary than that an animal, whose senses appear to be of the most imperfect description, should laboriously and patiently drill through a shell to obtain its food; and in the whole range of human and comparative anatomy, I am acquainted with no structure more complicated than the instrument by which this penetration is effected. The fact itself is noticed by PLINY; and although it has been questioned by some modern naturalists, while I am not aware that any have confirmed it by their own observation, it may yet be witnessed on the shores almost at any time. One of our most common littoral mollusca, *Buccinum lapillus*, feeds in this manner; and whenever it is seen attached to another shell-fish, with the foot slightly projected and expanded, a more or less advanced perforation will be found. On the shores at Swansea, its common prey is the muscle; and it sometimes, though rarely, attacks the oyster and anomia. Around Falmouth, it feeds chiefly on the limpet, but will occasionally be seen upon Turbo, Trochus, Nerita, and even its own species*.

The perforation is effected by a succession of strokes, following each other at intervals shorter than a second. I have distinctly heard them by applying to the ear a Patella which I had carefully removed from the rock, with a Buccinum attached to it. The process is extremely slow. I have found it still incomplete after having watched it for some hours. When the perforation has been effected, the prey is not immediately destroyed by any poisonous secretion; at least, I have kept alive for some days a muscle which the Buccinum had begun to eat. The trunk is therefore projected at first through the hole which it has drilled. But when, from the death of the animal, the limpet separates from the rock, or the bivalve gapes, the Buccinum devours the remainder by the natural opening.

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* Mr. DILLWYN's observations lead him to suppose that *Buccinum lapillus* commonly feeds on the Balanus. I have never seen anything to confirm this opinion, and believe the prey to be much too small for the full-sized Buccinum; but I constantly observe small specimens in situations where they could scarcely obtain any other food.

The slow penetration of *Buccinum lapillus* is explained by the weakness of the instrument, which is so small that I have not been able to dissect it. My description of this extraordinary weapon must therefore refer to that of *Buccinum undatum*, in which the parts are sufficiently large to admit of being shown distinctly.

Since this species undoubtedly feeds on carrion,—for it takes the fishermen's baits, while, from its semipelagic habits, it is never seen in the act of boring a shell-fish,—some proof will be required that it really feeds in this manner. It would probably be sufficient to state, that the shores of sandy bays, in which *Buccinum undatum* abounds, are strewed with immense quantities of perforated shells of the bivalves inhabiting sand; and that the perforations in these are much larger than could be effected by *lapillus*, which indeed is never found upon sandy shores. But I once obtained a decisive proof, in witnessing a *Buccinum undatum* discharge with its fæces the extremity of the foot, and the tubes of a *Lutraria compressa.*

Cuvier, in his Anatomy of this animal, has given a description of its boring trunk, illustrated by six figures; and I may be required to explain why I go over the same ground. It will be sufficient to state, that his description of all the more essential parts is vague, defective, and erroneous. The cartilages he represents, fig. 12, have no existence, and several of the most important muscles are overlooked. His different figures are not even consistent with each other. Thus, in fig. 10. the opening of the trunk is represented as a vertical slit, forming a pair of armed lips, and he describes it accordingly at p. 3; while in fig. 7, 8, and 9, it is correctly shown as a terminal and circular orifice.

The tongue of *Buccinum undatum* is about an inch in length, strap-shaped, and set with three longitudinal rows of teeth, which are short and straight in the centre, but large and hooked on either side, forming a perfect centre-bit. The disposition of the teeth is shown in fig. 18. The portions of the tongue which support the outer rows of teeth fold over upon the centre, and allow the instrument to be conveniently received within a membranous sheath. Into this sheath the muscles are inserted; and the tongue, issuing from its opening, is expanded and stretched over two cartilaginous points. A pair of small muscles inserted into the extremity of the tongue maintain it firmly in its position.

The tongue, with its apparatus of muscles, is contained within a strong membranous tube, which, at its posterior extremity, is doubled back upon itself; thus presenting a containing and a contained portion, so disposed, that in projecting the trunk, the contained portion is elongated at the expense of the other. The trunk is projected by a series of annular muscles, closely set along the whole of the containing tube; and it is retracted by a multitude of longitudinal ones, which, arising from either side of the cavity of the body, are inserted along this tube over their antagonists. The active extremity of the tongue is embraced and projected by a funnel-shaped muscle, arising from around the aperture of the tube, and which, at its upper part, is blended with the pharynx. The œsophagus rests upon the muscles of the tongue, and issuing from the extremity of the trunk, is doubled, and runs forward as far as the origin, or attachment of the containing tube; then, forming a second double, it passes back to the stomach. Such a mechanism was necessary, to allow the œsophagus to be projected with the trunk.

The muscular apparatus of the tongue is supported by a part which I shall call "the base." It presents the section of a cylinder, secured by two muscular crura. Its structure is chiefly membranous, with transverse muscular fibres, and with a double muscular column on either side. The inner columns are united at about a line from the point of the base, and their margins are free along their whole length; but the outer columns extend to the extremity of the base, and being tipped with cartilage, form the support over which the tongue is stretched. The opposite margins of the base itself are connected with transverse muscular bands, beneath which they give origin to five pairs of oblique muscles, which are inserted into the sheath of the tongue. A mass of longitudinal muscles pass between the crura of the base to be inserted into the back and sides of the sheath.

After this general description, the mechanism of the trunk will be sufficiently understood by a reference to the figures which illustrate the successive stages of a dissection. In fig. 14. we have the Buccinum cleared from the spire, mantle, and branchiæ, and laid open in a direction corresponding with the longitudinal axis of the body. The trunk is displayed in situ, with its extremity issuing from the containing tube, and resting at the aperture of the mouth. The annular muscles are seen on the containing tube, the last of the series being distinguished from all the others by its size and colour. The origins of some of the muscles of the trunk are perceived on either side.

In fig. 15. the trunk is projected to its full extent. The curvature and unequal thickness of the extended portion are quite characteristic. The multitude of muscles which arise from either side of the body, and especially from the right side, are seen entering the extremity of the trunk, while the œsophagus passes out from underneath them.

Removing the trunk from the body, and opening it down the side, we shall display the upper part of the tongue, with its muscles, as in fig. 16. The œsophagus is thrown with the divided tube to the left side, and the tortuous salivary ducts are seen passing along its under surface to the pharynx. Behind is the great annular muscle, *d*, with the mass of muscles which run forward to be inserted along the tube and into the tongue. The muscles where they arise from the body, as well as all those inserted into the tube, have the pearly bluish tint common to the muscular fibre of fishes; but the great annular muscle, and all inserted directly into the tongue, are of a red colour. Anteriorly at *e*, is the funnel-shaped muscle which projects the active portion of the tongue. The base is marked *a*, one of its crura *b*, and the muscular bands which connect its opposite margins *c*. Under these transverse bands, and issuing from behind them, is the sheath of the tongue, tortuous, and with a considerable muscle attached to its extremity; while beneath it, and within the crus of the base, are the long muscles which are inserted into it. The thin flat muscle *h*, given off on either side from near the extremity of the tube, and taking a somewhat oblique direction backward to be inserted into the base, probably assists in projecting and rotating the tongue.

Fig. 17, in which the tube is opened along its under part, displays many of the muscles represented in the preceding. They are distinguished by the same letters. The posterior part of the tube is contracted into annular folds by the corresponding muscles; and anteriorly, the tongue, having been stretched over the cartilaginous points of the base, is doubled back, its extremity being concealed under the insertion of the funnel-shaped muscle. A highly magnified view of this part of the tongue is given in fig. 18.

Reverting to the state of the parts in fig. 16, we divide the transverse bands of the base, and thus display the internal parts as in fig. 19. The sheath of the tongue is now fully exposed, with four pairs of oblique muscles inserted into it; of which one pair, *i*, take a direction backward, the others, *k*, *k*, forward.

Finally, in fig. 20. all these muscles are cut, and the tongue itself thrown aside. A deeper-seated pair of broad oblique muscles, *l*, and the insertions of the longitudinal ones are thus brought into view; while the internal structure of the base itself, with its muscular columns, and the cartilage with which the external ones are tipped, may now be conveniently examined.

The trunk of *Buccinum lapillus* must not be supposed to differ from that of *undatum* only in its size. It is essentially distinct in many points; but I shall not attempt a description on the accuracy of which I could place no reliance. The trunk of *Murex echinatus* appears to be of the same kind; presenting but a small mass of muscles at the very extremity of the tube. Some of the large tropical Murices will probably enable us to determine the anatomy of this variety. In the trunk of *Buccinum reticulatum*, we may trace without difficulty a very close conformity to the type of *undatum*, though the diameter of its muscular apparatus does not exceed that of a small pin.

There is another branch of the subject, into the details of which I shall not enter at present, but whose importance may claim a brief notice. In the modern systems of conchology, a beaked shell is considered to indicate a carnivorous animal; while an entire aperture is regarded as an equally unexceptionable mark of a herbivorous one. The first, I believe, is not to be disputed. There appears indeed no necessary relation between a respiratory tube and a boring trunk; and it may be curious to inquire why some of the carnivorous trachelipodes, *Buccinum undatum* and *reticulatum*, Cypræa, and others, carry their respiratory tube projected in an arch; while in *Buccinum lapillus* and Murex, it is lodged in a channeled beak: but there can be little doubt that all the beaked spirivalves are predatory. The opposite conclusion however is quite untenable; and the well-known example of Ianthina would alone be sufficient to overturn it. Although this molluscum cannot pierce shells, as

Cuvier states, for the obvious reason that it is itself the only floating shell-fish; and although its trunk is very unlike that of *Buccinum undatum*, to which he has compared it, its anatomy can leave no doubt of its carnivorous habits. Yet its aperture is entire, for the absence of anything like a respiratory tube forbids the extension of the columella from being considered as a beak.

Or if the columellar extension in Ianthina should be held to destroy the value of the exception, the aperture of *Natica glaucina* is perfectly entire, and this molluscum is certainly carnivorous. It devours the baits set by fishermen near low water mark; its fæces are slimy, and it is furnished with a considerable trunk, which bears a close resemblance to that of *Buccinum lapillus*, except in being less projectile, and is actually larger in proportion to the size of the animal. It is but reasonable to suppose that many other mollusca, marked with the same external characters, possess a similar structure and similar habits; and consequently, that the presence or absence of a beak is too exceptionable to be received as a distinction between the carnivorous and the herbivorous classes.

I suspect both Ianthina and Natica to be insectivorous. The latter is nearly a pelagic animal, and is never met with far from low water mark, except when thrown on shore by storms. The foot is large, composed of several lobes, and capable of being injected with water; and the animal is usually found, when under water, with the shell buried in the sand, while the injected foot rests like a mass of dead fish on the surface. May not this be a bait, to attract the prey which the animal is unable to pursue; and is it not probable that the extraordinary vesicular appendage of Ianthina may have a similar object?

This view of the subject receives support from the situations in which the animals are found. Floating helplessly on every part of the ocean, it would appear that Ianthina can obtain no food but the insects decoyed within its reach. The sandy bottoms, which are the haunt of Natica, afford no marine plants; it would rarely obtain carrion; and its tongue, closely studded with rounded tubercles, appears not at all calculated to penetrate shells.

To determine with exactness the anatomy of the organs of feeding in these animals, as well as of boring-trunks analogous to that of *Buccinum lapillus*; the nature and action of the digestive organs in the Bulla tribe; and the mode of feeding in the different land and fresh-water mollusca, will probably complete the general outline of this branch of zoology. Should leisure and opportunity allow, I shall hope to pursue the investigation.

I must not conclude without acknowledging my very great obligations to Mr. Dillwyn. His observations on fossil shells, published in the Philosophical Transactions, first suggested the inquiry; and the use of his valuable library, and still more, his own extensive information, have materially assisted me in the execution of it.

Explanation of the Plate.

Plate XIV.

Trochus crassus.

Fig. 1. The cartilaginous skeleton of the jaws.
Fig. 2. Upper view of the jaws, with the tongue and its muscles.
Fig. 3. Under view; the ligament of the jaws divided to show the insertion of the retractor muscles of the tongue.
Fig. 4. The stomach, laid open to display the contained œsophagus.

a. The accessory cartilages.
b. The muscles which expand the jaws.
c. Portions of them which project the tongue.
d. The transverse muscle which closes the jaws.
e. The retractors of the tongue.
f. A muscle passing from the tongue to the floor of the cavity.
g. A small muscle which assists to expand the jaws.
h. The lips of the stomach.
i. The valve in front of them.

Turbo littoreus.

Fig. 5. The fleshy mass of the mouth, with its muscles, the tongue, and the stomach, in situ.

Fig. 6. The pharynx detached, and the stomach turned forward, to display the tongue stretched over its elastic cushion.

Fig. 7. The elastic bodies which form the cushion.

a. The fleshy mass of the mouth.
b. The muscle which acts upon its base.
c. The muscle of the tongue.
d. The muscle of the sphincter.
e. The tongue.
f. Its termination in a spiral.
g. The stomach.
h. The muscles of the pharynx.
i. The earlike processes of the œsophagus.

Patella vulgata.

Fig. 8. The skeleton of the jaws.
Fig. 9. The upper jaw.
Fig. 10. The jaws, with the tongue and its muscles, in situ.
Fig. 11. Under view of the muscles.
Fig. 12. The retractor muscle of the tongue.
Fig. 13. Insertions of the muscles of the upper jaw.

a. The accessory cartilages.
b. The upper jaw.
c. The muscles of the pharynx.
d. The depressors of the upper jaw.
e. One of its levators.
f. The extensors of the tongue.
g. The transverse fibres of the retractor, which compress the jaws.
h. The columnar portions of the retractor.
i. Portions of the retractor which pass round the posterior cartilages.
k. Muscles which throw forward the accessory cartilages.

l. The lingual papilla. (The insertion of its retractor muscle is seen underneath it in fig. 11.)
m. The point of the tongue.
n. The soft lips.

Buccinum undatum.

Fig. 14. The trunk, in situ.
Fig. 15. The trunk developed.
Fig. 16. Upper part of the tongue, and its muscles.
Fig. 17. Under view.
Fig. 18. Magnified extremity of the tongue.
Fig. 19. Oblique muscles of the tongue.
Fig. 20. The base of the tongue.
a. The base.
b. Its crura.
c. Its transverse muscular bands.
d. The great annular muscle.
e. The funnel-shaped muscle which projects the tongue.
f. The muscle of the sheath.
g. The muscles of the point of the tongue.
h. The external oblique muscles.
i. The descending oblique.
k. The ascending oblique.
l. One of the deep-seated oblique muscles.
m. The sheath of the tongue.
n. The œsophagus.
o. Origin of the membranous tube which contains the tongue.

Of these, *a*, *b*, *c*, *e*, *g*, *h*, *k* and *l*, are not at all noticed by CUVIER, and the nature of *f* is mistaken.

Fig. 6, 7, and 18, are magnified; and 4, and 11, are a little larger than natural. The others are all of the size of the specimens from which they were copied.

XXII. *On the Mammary Glands of the* Ornithorhynchus paradoxus. *By Mr.* RICHARD OWEN. *Communicated by* J. H. GREEN, *Esq. F.R.S.*

Read June 21, 1832.

THE extraordinary nature of the monotrematous quadrupeds of Australia cannot be illustrated more forcibly than by observing that it is still doubtful to what class of animals they properly belong. In the confines of the animal kingdom, it is less surprising that a species should occasionally be discovered, either so devoid of external character, or of a form so strange, as to occasion a difficulty in ascertaining its class; and an Entozoon, a Lernæa, or an aggregate species of Salpa may require very minute investigation in order to determine its relation even to the most comprehensive division of a methodical arrangement. But the same difficulty occurring with respect to a hairy quadruped, affords one of the most unexpected, as well as interesting problems in natural history, and renders acceptable the smallest addition to the series of facts already ascertained respecting so anomalous a creature.

In this country we can hardly hope to throw light upon the economy of Ornithorhynchus and Echidna, except by the way of anatomy; at least, the aquatic habits of the former species render it improbable that it will ever be brought alive to our menageries. But the same objection does not apply to the spiny ant-eater, and it is to this animal therefore that the attention of voyagers from New South Wales should be more especially directed with a view of importation.

It is well known that one of the points now at issue with respect to these animals, is the nature of certain glandular organs which they possess, which are supposed to appertain to the mammary system: and it is obvious that our knowledge of the true affinities of the Monotremata greatly depends on a complete elucidation of this subject. To it, therefore, my attention has been particularly directed whenever an opportunity has occurred of examining the *Ornithorhynchus paradoxus;* and I have invariably noted the condition of the uterine organs with reference to that of the glands in question. In this way a series of facts has been ascertained, which I have ventured, from the interest of the subject to which they relate, to submit to this learned Society. But as the value of these observations, in a great measure, arises out of the state of doubt in which the question was left by previous researches, I have premised a short abstract of the anatomical history of the Monotremata.

Echidna Hystrix and *Ornithorhynchus paradoxus* were first described and figured by Dr. Shaw; the former, as early as the year 1792, in the third volume of the "Naturalist's Miscellany," under the denomination of *Myrmecophaga aculeata;* the latter, in the tenth volume of the same work, in 1799, by the name of *Platypus anatinus.* In the following year this extraordinary animal received a further description, together with its present generally adopted appellation, from Professor Blumenbach; and about the same time, Sir Everard (then Mr.) Home gave an account of some of its anatomical peculiarities, which appeared in the Philosophical Transactions for the year 1800. As these observations however were limited to the head and beak of the Ornithorhynchus, they threw but little additional light on the situation of that animal in the natural series. In the meanwhile, Professor Blumenbach placed the Ornithorhynchus among the Palmata of his system of natural history, intermediate to the otter and the walruss; while Dr. Shaw more correctly referred it to the Bruta of Linnæus; and, although limited to such traces of affinity as the outward form alone presented, he announced the alliance of this species, as well as of the Echidna, to the Myrmecophagæ.

The important memoirs on the anatomical structure of both these animals by Sir Everard Home, which were read before the Royal Society, and published in the Philosophical Transactions for 1802, drew the attention of the scientific world more strongly towards their remarkable peculiarities and deviations from the normal type of the Mammalia. In these investigations, the author, having brought to light numerous instances of mutual affinities before concealed beneath very dissimilar exteriors, grouped the two animals together under the same generic appellation. He also announced his opinion that they differed considerably in their mode of generation from the true Mammalia, grounding his belief on the peculiarities of the organs themselves, and on the absence of nipples in both species, and especially in the female of the *Ornithorhynchus paradoxus.*

The opinion of Sir **Everard Home** was soon after adopted by Professor **Geoffroy St. Hilaire**, who, in the Bulletin de la Société Philomathique, tom. iii. p. 225, constituted a new order for these animals under the term "Monotrêmes," being induced to believe, from an imperfect dissection, that the genital products of both sexes, as well as the urine and excrement, were voided by a common outlet*. Concluding also by inference that the mammary glands as well as nipples were wanting, and strengthened in his belief of the oviparous nature of the Monotremata, by some accounts from New South Wales of the discovery of the eggs of the Ornithorhynchus†, he subsequently separated the monotrematous animals altogether from the Mammalia, and characterized them as a class intermediate to Quadrupeds and Birds. (Bulletin de la Société Philomathique, tom. viii. p. 95.—Annales des Sciences Nat. xviii. p. 164.) The same idea had previously been entertained by **Lamarck**, (Philosophie Anatomique, 8vo, tom. i. pp. 145, 342); and by **Van der Hœven**, (Nova Acta Physico-medica Acad. Nat. Cur. tom. xi. Part. II. p. 368). But with these naturalists also the proposed dismemberment was founded principally on the presumed absence of mammary organs, unsupported by any additional facts relative to the internal anatomy of the species in question.

This mode of viewing the Monotremata was not, therefore, generally assented to. Possessing so many peculiarities of structure, these animals could not fail of attracting due attention from the immortal **Cuvier**. With his usual sagacity, he had very early perceived the true nature of the relation in which the *Myrmecophaga aculeata* of **Shaw** stood to the genus it was then placed in, and accordingly in the "Tableau Elementaire de l'Histoire Naturelle," (1797,) he separated it from the true ant-eaters, under the denomination of Echidna. He subsequently made considerable additions to the anatomical history of this species as well as to that of the Ornithorhynchus, acknowledged their mutual relations, and adopted the collective term proposed by Professor **Geoffroy**, but admitted it in the Règne Animal, as indicative of a tribe or family only, in his order Edentata.

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* See on the contrary the description of the male organs of the Ornithorhynchus, by Dr. **Knox**, in the fifth volume of the Wernerian Transactions, p. 152, where Sir **Everard Home**'s account of the passage of the seminal fluid by a distinct channel through the penis is confirmed.

† See Mr. **Hill**'s Letter in the thirteenth volume of the Linnean Transactions, inserted in the Mém. du Museum, tom. xv. p. 622: and that of Professor **Grant** in the eighteenth volume of the Annales des Sciences Naturelles, p. 161.

Oken and **De Blainville** more decidedly opposed the opinion of **Geoffroy**. The former naturalist even went so far as to hazard a conjecture respecting the mammary glands, and suspects that they will be found in the Cloaca, (Zoologie, tom. ii. p. 957); and M. **De Blainville**, in a dissertation on the place which the Ornithorhynchus and Echidna ought to hold in the natural series, after adducing the numerous instances in which the structure of the Monotremata agrees with that of the Mammalia, also expresses his belief that the mammary organs will ultimately be detected, and is of opinion that the animals themselves are most closely allied to the Marsupial order. Lastly, Professor **Meckel**, of Halle, announced in **Froriep's** Notizen, (Band vi. p. 144. 1824.) and subsequently in his excellent monograph on *Ornithorhynchus paradoxus*, (folio, Lipsiæ, 1826,) the existence of mammary glands largely developed in the female of that species *. In the latter work he accurately describes their situation, magnitude, form, and lobular composition. The tissue of the lobules he regards as consisting of closely aggregated tubes. Being unable to inject the gland, he is uncertain as to the precise mode in which the ducts terminate; but describes some small eminences, situated in the middle of the areola, as being undoubtedly orifices of the ducts.

From this most important example of the affinity of the Ornithorhynchus to the ordinary Mammalia, Professor **Meckel** is, however, far from drawing conclusions as to the identity of their mode of generation. For observing, "that the difference between the bringing forth of living young and of eggs is really very small, and by no means of an essential nature,—that birds have accidentally hatched the egg within the abdomen, and so produced a living fœtus,—an occurrence which has also been induced by direct experiment †,—and that, lastly, the generation of the marsupial animals is very similar to the oviparous mode," he deems it "very probable that, as the Ornithorhynchus approaches still nearer than the Marsupiata to Birds and Reptiles, its mode of generation may be in a proportionate degree analogous*."

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* This description has been translated into the French language and published by **De Blainville** in the Bulletin de la Société Philomathique, tom. ix. p. 138: and into our own language by the Editor of the second edition of **Lawrence's** translation of **Blumenbach's** Comparative Anatomy.

† Probably in allusion to the Experiences sur la Génération des Animaux Ovipares, par M. **Rossi**, Mém. de l'Acad. de Turin, 1779, p. 266.

For an animal possessing mammary glands he claims, however, the right to rank with the Mammalia; and accords with Professor Geoffroy only so far as to consider the Monotremata a distinct order of quadrupeds, which he places, as in the system of Cuvier, next to the Edentata.

Notwithstanding the authentic and circumstantial manner in which this discovery was given to the world, it has been by no means universally regarded as conclusive with respect to the mammiferous nature of the Monotremata. Professor Geoffroy, having subsequently had an opportunity of dissecting a female Ornithorhynchus, and of verifying in some measure the description above quoted, has more especially endeavoured to invalidate the inferences drawn from it. He urges †, that the subcutaneous abdominal glands considered by Meckel as mammary, possess none of the characters of a true mammary gland;—that he examined them with the greatest attention, comparing them with the human mammary glands, and especially with those of marsupial animals, and that they were of a totally different texture (*tissu*), consisting of a vast number of cæcums placed side by side, all directed to the same point of the skin, where only two excretory orifices were to be perceived, and these orifices so small, that the head of the smallest pin could not be made to enter them;—that, above all, there was no trace of nipples;—that in the specimen he examined, which had the size and appearance of an adult female Ornithorhynchus, the apparatus in question was not more than a fourth part of the size of that observed by Meckel. But a mammary gland, he further observes, when arrived at its full development, occasions an enlargement of all its constituent parts, the nipple acquiring additional bulk even before lactation commences, while nothing of the kind has been noticed in the Ornithorhynchus. He considers them, therefore, as being analogous rather to those glands for the secretion of a lubricating fluid, that are disposed along the flanks of the aquatic reptiles and fishes; or to the odoriferous follicles of quadrupeds, and especially to those which are found on the sides of the abdomen in shrews. To these objections Professor Meckel has replied in his Archiv für Anatomie und Physiologie, B. x. p. 23; where, after combating the arguments drawn by Professor Geoffroy from the supposed follicular structure of the glands and the absence of a nipple, he particularly urges the great difference of size which the glands presented in the two females examined, and also their total absence in the male,—both which circumstances he considers as strongly corroborative of his original opinion. In the same work (B. x. p. 568,) Professor V. Baer, in support of the opinion of Meckel, adduces the example of a mammary gland analogous in simplicity of structure to that of the Ornithorhynchus, viz. in the Cetacea, where its function has never been questioned. But as no additional particulars relative to the structure of the glands in the Ornithorhynchus have arisen out of this discussion, I shall not dwell further on the arguments used by these celebrated anatomists, but proceed to give the results of my own investigations relative to this subject.

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* Ornithorhynchi paradoxi Anatome, fol. p. 58.

† Annales des Sciences Naturelles, tom. ix. p. 457.

In five apparently adult and full-grown Ornithorhynchi examined by me, the mammary glands presented as many different degrees of development. In one of the specimens they were even larger than in that dissected by Meckel, measuring in length respectively five inches and a half, in breadth two inches, and in thickness from four to five lines. In another specimen they did not exceed one inch and a half in length, and were only five lines in breadth, and half a line in thickness. In the remainder the mammary glands were of intermediate sizes to the two above mentioned.

In each specimen the gland was composed of from one hundred and fifty to two hundred elongated subcylindrical lobes, disposed in an oblong flattened mass, and converging to a small oval areola in the abdominal integument, which areola is situated between three and four inches anterior to the cloaca, and about one inch from the mesial line. The lobes in the smaller glands preserve the same breadth to near their points of insertion, but in the larger ones they are broadest at the free extremity, measuring three or four lines across, and become narrower to about one third from the point of insertion, where they end in slender ducts. The lobes are almost all situated to the outer side of the areola, and consequently converge towards the mesial line of the body.

Between the glands and the integument the panniculus carnosus is interposed, closely adhering to the latter, but connected with the glands by loose cellular membrane. This muscle is here nearly a line in thickness; its fibres are longitudinal, and, separating, leave an elliptical space for the passage of the ducts of the gland to the areola. (Pl. XVIII. fig. 1.)

On the external surface of the skin the areola (when the hair with which it is covered has been removed,) can only be distinguished by the larger size of the orifices of the ducts as compared with those for the transmission of the hairs, and occasionally by being of a deeper colour than the surrounding integument. The orifices of the ducts thus grouped together form an oval spot, which in the specimen which had the largest glands measured five lines in the long, and three in the short diameter. In that in which the glands exhibited the smallest size, the areola could be traced by the aid of a lens to nearly the same extent in the long diameter, but it was much narrower. From the minuteness of the orifices of the ducts in the specimens with the small glands, the situation of the areola can hardly be detected without previously dissecting the gland; whilst in those in which the glands are fully developed, the practised eye readily discovers the areola on removing the hair. In none of the specimens was the surface on which the ducts terminated in the slightest degree raised beyond the level of the surrounding integument; the elevation like a millet-seed in Professor MECKEL's specimen I conceive to have been accidental, and not essential to the structure of the part, having observed similar risings in the integument at different distances from the areola, but not in the areola itself. The orifices, moreover, appear of nearly equal sizes, not any of them at least being calculated to suggest the idea of its being common to many ducts or lobules, as might be inferred from the description of Professor GEOFFROY. (The appearance which one of the areolæ presented under the microscope is represented at Pl. XVIII. fig. 2.) On compressing the glands in a specimen in the Museum of the Zoological Society, where they had arrived at the maximum of development, there escaped from these orifices minute drops of a yellowish oil, which afforded neither perceptible taste nor smell, except such as was derived from the preserving liquor.

Having in vain attempted to insert the smallest absorbent pipe into these orifices, I thrust it into the extremity of a lobule, and after a few unsuccessful efforts at length perceived the mercury gradually diffusing itself in minute globules through the parenchyma of the lobule; and at the distance of an inch from the place of insertion it had evidently entered a central duct, down which it freely ran to the areola, where it escaped externally from one of the minute orifices just described. This process was repeated on most of the lobes with similar results; the greater part of them terminated by a single duct opening exteriorly, distinct from the rest; but in a few instances the ducts of two contiguous lobules united into one, and in these cases the mercury returned by the anastomosing duct, when the common one was tied up, and penetrated the substance of the other lobule as freely as that into which the pipe had been inserted.

Some of the lobules injected by the reflux of the mercury through the anastomosing duct were dried, and various sections were submitted to microscopical examination. At the greater end the lobules are minutely cellular; these cells become elongated towards the centre of the lobule, and as it grows narrower, form minute tubes which tend towards, and terminate in a larger central canal, or receptacle, from which the excretory duct is continued. When uninjected, the entire lobule might be readily supposed to be composed of minute tubes; but it is difficult to imagine how the lobules can have been considered as hollow cæcums or elongated follicles. On making a section of the corium through the middle of the areola, the ducts were seen to converge in a slight degree towards the external surface; but there was no trace of an inverted or concealed nipple, as has been observed in the kangaroo. (Fig. 5. Pl. XVIII. represents a magnified view of this section, with a section of one of the dried and injected lobules.)

The next stage of the inquiry was the examination of the ovary and other organs of generation in the specimens which had presented such a diversity of size in the mammary glands; and as they exhibited in these dissections corresponding differences of development, the following account of the structure of the uterine organs may not be wholly unacceptable, notwithstanding the extended memoir on the subject inserted by Professor GEOFFROY in the Mémoires du Muséum, tom. xv. p. 1.

There is no part in the female Ornithorhynchus that can be properly termed vagina; but the canal which leads from the orifices of the uteri to the external outlet may be divided into two portions: of these the first and most internal is termed by Professor GEOFFROY the *urethro-sexual canal*, as it conveys the urine and the genital products into the second or external cavity: for this part he retains the name, originally given to it by Sir EVERARD HOME, of *vestibule*, as it affords a common outlet to the preceding substances and the contents of the rectum.

The common vestibule is about one inch four lines in length, and varies from half an inch to an inch in diameter. The muscular fibres immediately investing it are disposed as follows. A thin circular muscle arises from a dorsal raphé which extends the whole length of the canal. Of this muscle the sacral fibres, or those nearest the outlet, surround the whole vestibule; but the atlantal or more internal fibres pass obliquely upwards and surround the termination of the rectum only, serving as a sphincter to it. On the sternal aspect of the vestibule there are a series of longitudinal fibres, which extend from its external orifice to that of the urethro-sexual cavity, the office of which is to approximate these orifices, and in this action the oblique fibres above described would assist, while at the same time they closed the rectum.

On the sternal aspect of the urethro-sexual cavity, and close to where it joins the vestibule, the clitoris is situated, which is consequently about an inch and a half distant from the external orifice of the vestibule. It is inclosed in a sheath upwards of an inch in length, and about two lines in diameter, of a white fibrous texture, and with a smooth internal surface, and this sheath communicates with the vestibule about a line from the external aperture. The clitoris itself is a little flattened body shaped like a heart on playing-cards; it is about three lines long, and two lines in diameter at its dilated extremity, where the mesial notch indicates the correspondence with the bifurcated penis of the male. From the shortness of the clitoris, and the length of its sheath, it is obvious that no part of it can project into the vestibule in the ordinary state of the parts, as stated by Sir EVERARD HOME, its extremity being situated at least an inch distant from where its sheath communicates with that cavity. At the base of the clitoris are two small round flattened glands which open into the sheath or preputium clitoridis. These glands were largest in the specimen whose uterine organs were most developed. The vestibule is lined by a dark-coloured cuticular membrane, and has a tolerably uniform surface. The rectum opens freely into it posteriorly, the line of distinction in the relaxed state of the sphincter above mentioned being little more than a change in the character of the lining membrane. The urethro-sexual canal, on the contrary, opens into the vestibule by a contracted orifice, and in one of the specimens examined, made a small circular and valvular projection into that cavity. On either side the termination of the rectum there are from six to eight small apertures of dark-coloured glands or follicles, about the size of a pin's head, situated immediately behind the proper membrane of the vestibule.

The urethro-sexual canal is one inch and a half long, and three or four lines in diameter, but capable of being dilated to as great an extent probably as the pelvis will admit of, the diameter of that passage being seven tenths of an inch. It is also invested with a muscular coat, the external fibres of which are longitudinal; the internal, circular. The inner membrane of this part was disposed in longitudinal rugæ more or less marked, but presented as little the character of a secreting membrane as that of the vestibule, being smooth and shining; after a careful examination with the lens, the orifices of a few minute follicles were discovered in the interstices of the rugæ near the orifice of the urinary bladder.

It is this division only of the passage from the uterus which is situated within the pelvis, the vestibule being produced beyond it, and the common outlet being in consequence situated at a considerable distance from the outlet of the pelvis, as in the beaver, which besides its analogy in habits to the Ornithorhynchus is also in the literal sense of the word monotrematous. If, then, the Ornithorhynchus be really oviparous, its eggs must be disproportionately small compared with those of birds, in order to pass through the pelvis. For on the supposition that they are of "the size, shape, and colour of those of a hen *," the yelk at least must be much smaller; for it is obvious that this part only of such an egg could pass through the pelvis, and the albumen and shell must necessarily be laid on in the vestibule. But, as has been before observed, neither the lining membrane of the vestibule nor that of the genito-urinary passage presents the characters of a secreting membrane; and great alterations at least must take place in them, if they exercise any share in contributing to the nutrient store of the embryo.

At the atlantal extremity of the urethro-sexual canal there are five distinct

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* Linn. Trans. vol. xiii. p. 624.

orifices: the largest is in the middle and conducts into the urinary bladder; about three lines below this orifice are those of the uteri, situated, each on a small nipple-like prominence, or os tincæ; and just below these, but on the same prominence, are the terminations of the ureters. These prominences were most marked in the specimens with the largest ovary and uteri, and the one on the left side projects further than that on the right.

The uteri are two distinct tubes, not arising, like the horns of the uterus in ordinary quadrupeds, from a cavity peculiar to them, or corpus uteri; but continued, as in tortoises, from a cavity into which the urinary bladder and ureters separately enter. Neither is this the sole resemblance they bear to the oviducts of reptiles; for, compared with ordinary quadrupeds, the distinction between the true uterus and Fallopian tube is but slightly marked, and the entire canal is thrown into many convolutions, partly by the process of peritoneum, or ligamentum latum, which attaches them to the pelvic region, and partly by means of a ligamentous chord upon which the convolutions are, as it were, strung. In their natural state the uteri measure about three inches in length; but when the convolutions are unfolded they extend to more than double that length; the right uterus, however, being always the shortest. The ligament above mentioned arises from the posterior parietes of the abdomen in the situation analogous to that of the testes in the male, viz. below, and a little to the outer side of the kidneys, and passes along the edge of the broad ligament to the Fallopian extremity of the uterine tube, where it divides; one portion is continued along the posterior margin of the orifice of the uterine tube, the other along the corresponding edge of the ovary; and after a course of an inch they again unite, and the ligament is continued along the anterior part of the uterus to the neck of the tube, where it is insensibly lost. The two separated portions of the ligament support a large pouch of peritoneum, which forms the ovarian capsule; the wide anterior orifice of the uterus is also by means of this ligament prevented from being displaced or drawn away from the ovary, during the actions of the rest of the tube.

The structure of the uterine tube is the same on both sides of the body. It is enveloped in a loose external serous coat, connected to the muscular coat by filamentary processes of cellular membrane, among which, numerous tortuous vessels ramify. The second tunic is a thin compact membrane, which I conclude to be muscular from analogy only, having been unable, even with a high magnifying power, to detect a distinct arrangement of fibres in it. It is most easily demonstrated as a distinct coat in the dilated uterine portion of the tube. The innermost coat is a soft pulpy membrane with a slightly granular surface in the uterine portion of the tube, but thin and smooth in the Fallopian division. The difference was most considerable in the specimen with the largest ovary, in the uterine portion of which this membrane was thickened and of a dark colour, but no villi were perceptible on it when examined with the lens.

The left uterus, in the specimen with the large ovary, (Fig. 1. Pl. XVI. & fig. 3. Pl. XVII.) was for the first two inches of its extent from four to five lines in diameter, and about a line thick in its parietes; it then became suddenly contracted, and thinner in its coats, diminishing in diameter to about two lines, and afterwards slightly enlarging to within an inch of the extremity which forms a wide membranous pouch opening into the capsule of the ovary by an oblong orifice or slit of eight lines in extent. The edges of this orifice were entire, as in the oviducts of reptiles; not jagged as in the fimbriated extremity of the Fallopian tube in ordinary quadrupeds: nevertheless the dilated and muscular part of the tube at its commencement may be considered as the true uterus, and the contracted portion beyond as the Fallopian tube. The entire length of this uterus when detached from its connexions was nine inches. The right uterus in the same specimen exhibited similar differences in diameter and structure; but the contracted part representing the Fallopian tube was shorter in proportion to the true uterine division. This uterus measured six inches in length.

In the specimen with the smallest developed ovary, (Pl. XV. fig. 1.) the first portion of the uterine tubes was very little wider than the second, and not thicker in its coats; the entire tubes were much less in all their dimensions than those just described, and the terminal cavity, though more dilated than the rest of the tube, was also smaller.

In another specimen, in which the ovary (Pl. XVIII. fig. 4.) appeared to have shed its contents, the uteri presented the same variations of diameter as in the specimen with the largely developed ovary; but the parietes of the uterine portion were not so thick.

In the specimen above described with the large ovary, the thickened parietes of the first portion of the uterine tube depended chiefly on an increase of the inner membrane, which possesses in a high degree the character of a secreting membrane. This membrane at the cervix uteri presented in all the specimens many deep and close-set furrows, which as the canal grew wider were gradually lost, and the surface became more or less smooth in the different specimens, being most irregular in the specimen with the largest ovary: in the contracted part of the tube, the inner surface is at first smooth, but beyond that becomes finely reticulate, and in the terminal dilated part is again smooth. The laminæ at the cervix uteri, when seen from the urethro-sexual cavity projecting across the terminal orifice, give the appearance of that orifice being divided by a septum. But in whatever way I have examined this part, I have never been able to detect such a division; the uterine tubes have invariably presented only a single aperture of communication with the urethro-sexual cavity. Such a septum may, however, exist in the virgin state of the parts; and on their assuming the natural functions, it may, like the hymen, be obliterated. Professor GEOFFROY, who has described and represented this structure, (Mém. du Muséum, xv. p. 32. Pl. I.) regards it as a rudimentary indication of the form of uterus peculiar to the Marsupiata.

In all the specimens but one, the ovary existed only on the left side; it is appended to the portion of ligament* before mentioned, and is of a flattened oblong form. In the specimen in which the mammary glands presented the smallest size (Plate XV.), the left ovary consisted of a thin, smooth, and soft substance, measuring half an inch in length, three lines in breadth, and half a line in thickness; the external covering was a tough membrane, beneath which were two black specks, but there was no appearance of ova; the rest of the substance being cellular membrane only. In the specimen (Pl. XVI.) in which the mammary gland was a little more advanced than the preceding, the left ovary presented the highest observed degree of development; and the right ovary was more distinct than in any of the other specimens. The left ovary was nine lines in length, five in breadth, and from two to three in thickness, having numerous ova distinctly developed in it, two of which were two lines and a half in diameter; and therefore, probably, not less than those which Mr. HILL has described† as being the size of small peas. These consisted of a tough capsule filled with a soft substance of a dark brown colour. The remaining ova varied in diameter from a line to the fiftieth part of an inch, giving an irregular tuberculate surface to the ovary, and a superficial resemblance to the ovary or clutch in the bird: but in the Ornithorhynchus the ova are enveloped in a tough fibrous membrane, in which the traces of vascularity (at least after having been preserved in spirits,) are not perceptible, whilst in the fowl the ova are attached by narrow pedicles, and are covered by a thin and highly vascular membrane. The right ovary in this specimen was of an elongated form, attached to, and apparently developed from the ligament above mentioned; it was a thin substance about half an inch in length, and nearly two lines in breadth, with the surface studded over with incipient ova. This appearance renders probable the supposition of Sir EVERARD HOME that it may come into action at some period of the animal's existence; but the traces of it in all the other specimens could only be recognised in a slight thickening of the ligament. The mammary glands in this specimen were each two inches four lines in length, eight lines in breadth, and nearly a line in thickness. The lobules of the gland had increased more in length than breadth, being almost as narrow as in the smallest gland. In both instances they were of the same colour and texture as in the largest glands.

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* This ligament is represented in Mr. BAUER's magnified drawing of the posterior view of the ovary of the Ornithorhynchus, Phil. Trans. 1819, Pl. XVIII. p. 240.

† Linn. Trans. vol. xiii. p. 623.

In the specimens in which the mammary glands had arrived at their full size, the ovary presented the following appearance. It was nearly as large, as respects length and breadth, as in the preceding case, but was much thinner, and its surface was rendered irregular by furrows and wrinkles. There were also minute granules of a black colour immediately beneath the outer covering, but the body of the ovary was composed of a loose cellular texture only. It may reasonably be concluded, therefore, on a comparison of these appearances with those exhibited in the ovaries previously described, that they indicated the condition of the ovary shortly subsequent to the performance of its peculiar functions, and that at this period, the circulation having been diverted to the neighbouring mammary organs, had contributed to their excessive development.

In the female wherein the ovary and the uteri were in apparently the lowest stage of adult development, and exhibited no traces of recent action, the mammary glands presented a volume indicative of a corresponding degree of inactivity. Where the ovary had made a considerable advance towards perfection, the glands did not exhibit a corresponding degree of development; they had only begun to enlarge and to manifest their obedience to the law of the sexual impulse. But had their office been to secrete, as Professor GEOFFROY supposes, an odorous substance attractive of the male, their maximum of development ought to have been exhibited in this specimen, in which the uteri evinced, by their size and vascularity, traces of high excitement, and the ova appeared ripe for impregnation. The greatest development of the abdominal glands, on the contrary, was observed where the ovary appeared to have recently executed its function.

The variation in size of these glands, in individuals of the same bulk, evidently points out that they are not adapted for the performance of any constant office in the economy of the individual, but relate to a temporary function. Otherwise, the circumstance of their yielding oil on pressure, as in the instance above mentioned, might have led to the supposition that they furnished a lubricating fluid useful to an animal of the aquatic habits of the Ornithorhynchus *.

That this temporary function, moreover, is peculiar to the economy of the female, cannot be doubted. For in the male, both Dr. KNOX and Professor MECKEL have been unable to detect these glands; and after a careful scrutiny, with the same view, in a well preserved specimen of that sex, I have not succeeded in detecting more than a few minute lobules occupying a space of about four lines in situations corresponding to those in the female; but the nature of which, from the total absence of corresponding foramina on the external surface of the integument, may still be doubted.

Lastly, from the evidence derived from the uterine system in the present inquiry, the period when these glands exhibit the greatest activity, appears to be *after* gestation. It therefore comes to be considered whether their structure is so widely different from the ordinary mammary gland as it has been represented to be.

Now, whether the secretion of these glands be milk or not, it is highly probable, from its being conveyed externally by long and narrow ducts, that it is of a liquid nature; and this mode of being carried off is much more analogous to that exhibited in the ordinary lacteal apparatus than in the odoriferous glands, which more commonly open externally by one large and wide orifice. The excretory orifices of the glands in the Ornithorhynchus, moreover, are not extended over a wide surface, but are collected into a point, in all probability, not disproportionate to the size of the mouth in the young animal, and these points are situated in parts of the body most convenient for the transmission of a lacteal secretion from the mother to her offspring.

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* Since writing the above, I have ascertained that the mammary glands exist in a similar situation, and under a similar form, in the *Echidna hystrix*; an animal which burrows in dry sandy situations.

Compared with an ordinary mammary gland, that of the Ornithorhynchus differs chiefly in being of a cellular and not of an acinous or conglomerate structure; as well as in the absence of the nipple and of the surrounding vascular structure necessary for its erection. But the inconclusiveness of arguments drawn from these circumstances has been sufficiently demonstrated by Professors MECKEL and V. BAER in the work above quoted. The question then arises, how the secretion of this gland, if mammary, is conveyed to the young? And with respect to the absence of a nipple, Professor GEOFFROY observes, "C'est ainsi chez un animal dont le museau est fait de façon que même y aurait il une long tétine un tel animal serait privé de la saisir et de la sucer."

But with a form of mouth so extraordinary and unlike that of other quadrupeds, might we not expect some corresponding deviation from the normal structure in the efferent portion of the mammary apparatus? And if a nipple would indeed have been useless or unavailable in this case, have we not then the best reason for its absence? Unless, however, we limit nature to one mode only of conveying the lactiferous secretion from the parent to the offspring, I apprehend the evidence afforded by the preceding details will hardly render tenable any other theory than that which upholds the mammary nature of the glands in question.

Fortunately, an instance has already been afforded, and that too in the Marsupiata, of a structure superadded to the mammary gland apparently to compensate for a want of sufficient power of suction in the young animal*. So also in the Ornithorhynchus the strong panniculus carnosus which is every where interposed between the glands and the skin, may compress the glands against the flattened cartilages of the ribs and the marsupial bones, and occasion the expulsion of the secreted fluid; while the great extent and the yielding texture of the gland seem peculiarly to adapt it to be so influenced. In this case the mouth of the young animal need only be applied to the areola to receive the secretion; and it is particularly worthy of remark, that the great distinction between the mandibles of the Ornithorhynchus and those of the Bird consists in the former being, even in the adult state, surpassed by thick, soft, muscular, extremely sensible and flexible lips.

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* See Professor GEOFFROY's account of this apparatus in Mem. du Muséum, tom. xv. p. 48; and Description of the Mammary Organs of the Kangaroo, by JOHN MORGAN, Esq. Linn. Trans. vol. xvi. p. 61.

Appendix.

Whilst the preceding account was going through the press, the following interesting communication was made by Dr. Weatherhead to the Committee of Science of the Zoological Society.

After stating that he had received a letter from his friend Lieutenant the Hon. Lauderdale Maule of the 39th Regiment, at present in New South Wales, informing him of his having forwarded, among other curious objects of natural history, the carcases of the female Ornithorhynchus and her young, he proceeds to give the following extracts from Lieutenant Maule's letter:—"'Several of their (the Ornithorhynchuses') nests were with considerable labour and difficulty discovered. No eggs were found in a perfect state, but pieces of substance resembling egg-shell were picked out of the *debris* of the nest. In the insides of several female Platypi which were shot, eggs were found of the size of a large musket-ball and downwards, imperfectly formed however, i. e. without the hard outer shell, which prevented their preservation. Several young Platypi were obtained and put into spirits, in which state they are forwarded.'

"In another part of his letter Mr. Maule states, that in one of the nests he was fortunate enough to secure an old female and two young. The female lived for about two weeks on worms and bread and milk, being abundantly supplied with water, and supported her young, *as it was supposed*, by similar means. She was killed by an accident on the fourteenth day after her capture, and on skinning her while yet warm, it was observed *that milk oozed through the fur on the stomach*, although no teats were visible on the most minute inspection; but on proceeding with the operation *two teats or canals* were discovered, both of which contained milk. The carcase also of this female Mr. MAULE has kindly forwarded."

In the preceding account, therefore, two important facts are distinctly stated; the one, that the ova of the Ornithorhynchus attain the size of a large musket-ball, and, like the eggs of the ovo-viviparous reptiles, have a soft outer covering; the other, that the fluid secreted by the abdominal glands is milk. The first of these statements would of course derive additional value if the period of the year were stated when the eggs so developed were observed; and the precise part of the body in which they were situated, whether in the ovary, the oviduct, or the cloaca: also, whether they were observed at the same time that the female with her young was captured, or at what distance of time from that event.

With respect to the supposed portions of egg-shell found in the nest, it is obviously far from being conclusive as to the oviparous character of the Ornithorhynchus; since, when it is considered that the excrement and urine are expelled by the same orifice, we may readily suppose the former to be coated, as in birds, with the salts of the urine, and to have given rise to the above appearances.

The information respecting the mammary glands is much more satisfactory, and must be regarded as decisive of the question relative to their function. The mode of suckling appears, indeed, not to have been observed; but the ready escape of the secreted fluid after death, during the process of skinning, is corroborative of the opinion previously advanced as to the manner in which the milk is expelled. Among the other points of interest for which the scientific world is so highly indebted to the exertions of Lieutenant MAULE, that of discovering the number of young produced by the Ornithorhynchus may in all probability be reckoned; and it would appear, that, as in other Mammalia, it corresponds with the number of nipples, or outlets for the mammary secretion.

Fig. 1.

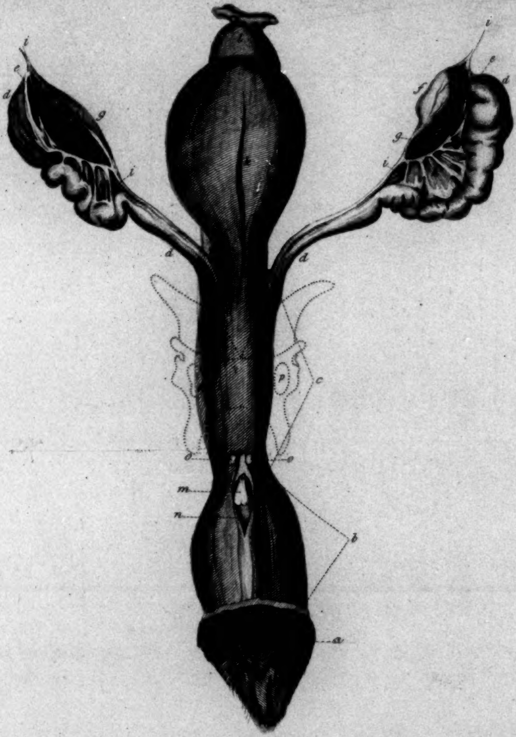
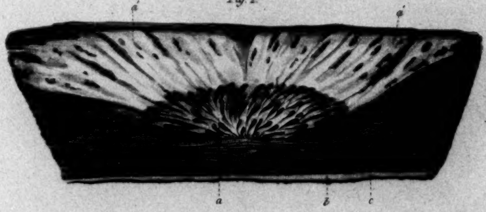


Fig. 2.



Richd. Owen, del.

J. Basire sc.

Fig. 1.

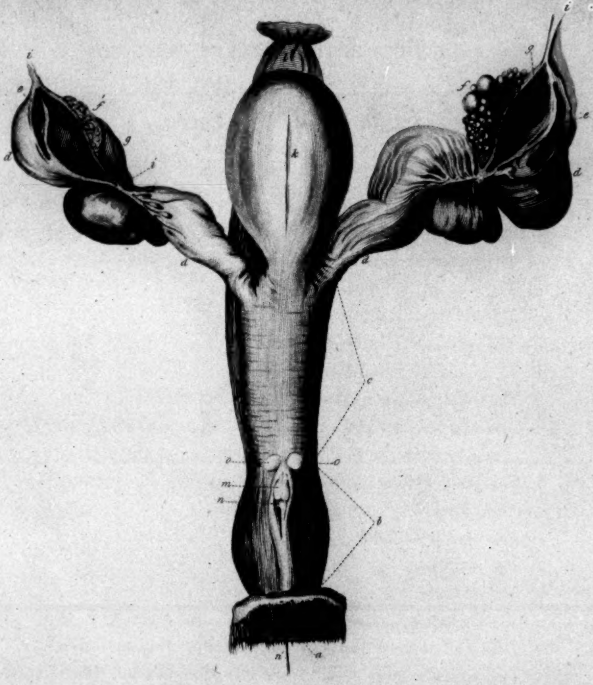
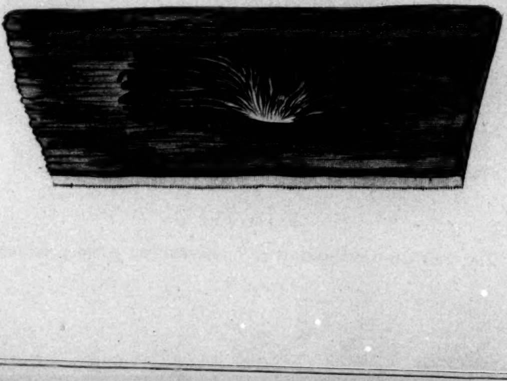


Fig. 2.



Richd Owen, del.

J. Basire sc.

Description of the PLATES.

PLATE XV.

Fig. 1. The organs of generation in the unexcited state, with the urinary bladder and rectum of a full-grown female Ornithorhynchus.

a. The external outlet or orifice of the vestibule.

b. The vestibule.

c. The urethro-sexual canal.

d, d. The uterine tubes or oviducts.

e, e. Their anterior or Fallopian orifices.

f. The ovary, developed only on the left side.

g, g. The ovarian capsules or peritoneal bags connecting the ovarian ligaments with the Fallopian extremities of the uterine tubes.

h, h. The processes of peritoneum connecting the oviducts to the ligaments *i, i.*

k. The urinary bladder.

l. The rectum.

m. The clitoris.

n. The sheath or preputium clitoridis. *n'.* Plate XVI. A bristle passed into the sheath through the orifice in the vestibule.

o, o. Two small glands which open into the sheath of the clitoris.

p. Outline of the pelvis, showing its relation to the urethro-sexual canal.

Fig. 2. One of the mammary glands from the same individual, exhibiting the lowest observed degree of development.

a. The gland. *d', d'.* Sheaths of cellular membrane which could be inflated, and had been occupied probably with the glandular lobules at a previous period of enlargement.

b. The panniculus carnosus.

c. The integument.

PLATE XVI.

Fig. 1. The organs of generation of another adult female, which were probably prepared for impregnation. The letters indicate the same parts as in the preceding plate: *f'* is the ovary, slightly developed on the right side.

Fig. 2. One of the mammary glands, from the same individual, beginning to enlarge.

Plate XVII.

Fig. 1. The same parts as are represented in fig. 1. of the preceding plate, but further dissected and laid open.

a. The common outlet or orifice of the vestibule.

b. The vestibule, with its anterior or sternal parietes removed.

b'. A probe passed through the rectum into the vestibule.

c. The urethro-sexual canal laid open.

c'. The orifice by which the urethro-sexual canal communicates with the vestibule.

d, *d*. The dilated or uterine portions of the oviducts laid open.

d', *d'*. The contracted or Fallopian portions: that on the left side is laid open through its whole extent, showing the dilated cavity at *d''*.

e, *e*. The wide slits which form the orifices of the oviducts.

f, *f'*. The ovaries. *i*, *i*. The ligaments which attach the oviducts and ovaries to the back of the abdomen.

k. The urinary bladder opening into the atlantal extremity of the urethro-sexual canal.

l, *l*. The ureters, through which bristles are passed to show their terminations in the urethro-sexual canal.

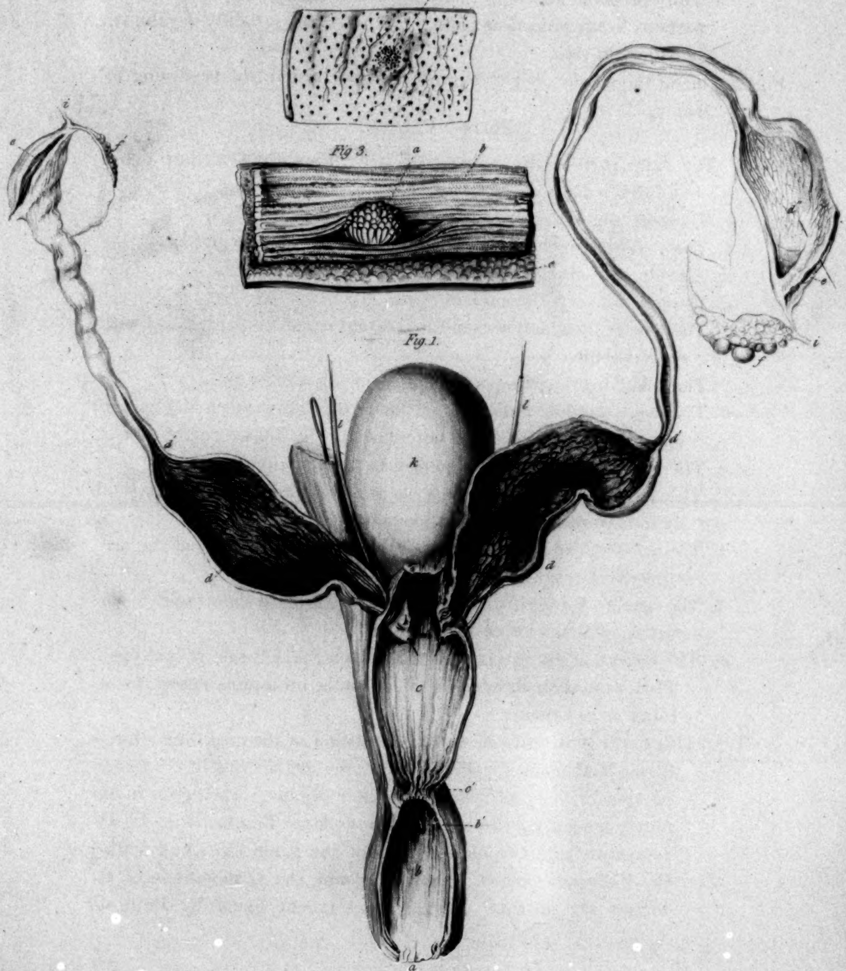
m. The orifices of the uterine tubes; that on the left side is laid open. They were each situated in this instance on a prominence resembling an os tincæ.

This figure is in some measure a repetition of the preceding; but is here added, as it supplies some of the deficiencies in the figures previously given of these remarkable organs. The figure in the ninety-second volume of the Philosophical Transactions, Pl. IV. represents both the uterine tubes of the same size; and neither the Fallopian orifices, the ovaries, nor the terminations of the ureters are shown. In the more recent figure by Professor

Fig. 2.

Fig 3.

Fig. 1.



Richd. Owen, del.

J. Basire sc.

Fig. 1.

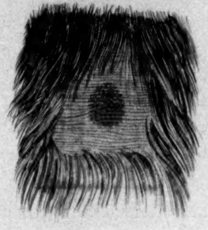


Fig. 5.

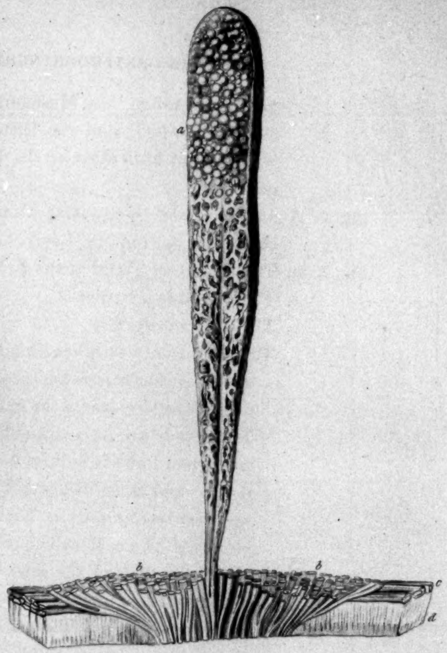


Fig. 2.

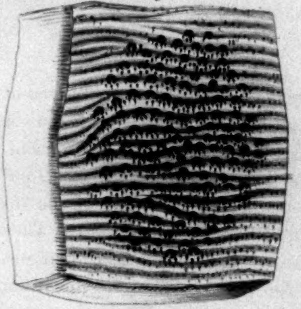


Fig. 4.

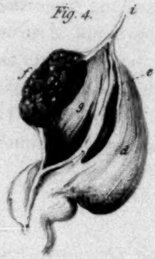
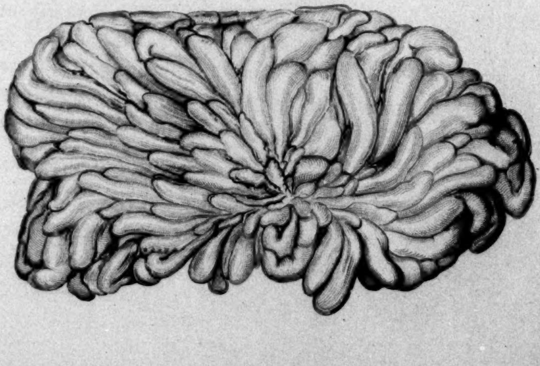


Fig. 3.



Richd. Owen del.

J. Basire sc.

Geoffroy (Mém. du Muséum, Pl. I. fig. 6.) the right uterine tube is omitted, and the left is made to terminate in a point without any indication of the Fallopian orifice or of the ovarian capsule.

Fig. 2. A portion of the integument from the abdomen of the Spiny Anteater (*Echidna hystrix*, Cuv.) showing at *a*, the mammary areola.

Fig. 3. *a*. The mammary gland of the *Echidna hystrix*.

b. The panniculus carnosus.

c. The integument.

This specimen was taken from a young female nearly arrived at maturity, but which had probably never been impregnated; it consequently exhibits the gland in a low stage of development. The glands are two in number as in the Ornithorhynchus, and are situated about half an inch from the mesial line of the abdomen, and three inches and a half anterior to the cloaca. They are each composed, as in the Ornithorhynchus, of numerous elongated lobes, which converge towards the mesial line, their ducts penetrating the integument, and forming by the aggregation of their orifices a small areola externally. This areola is more easily distinguished in the Echidna, from the hairs on the abdomen being more scattered; it is not situated on an eminence, nor surrounded by any erectile tissue: it is made up of about sixty orifices. The lobes of the gland are proportionally broader and shorter than in the Ornithorhynchus. A strong panniculus carnosus is similarly interposed between them and the integument, and the fibres of this muscle separate to allow the ducts to pass through, as represented in the Plate. The lobes are not mere cæcums, but present under the lens a similar texture to those in the Ornithorhynchus.

Plate XVIII.

Fig. 1. A portion of integument from the abdomen of the *Ornithorhynchus paradoxus*, with the hairs removed so as to exhibit the mammary areola.

Fig. 2. A magnified view of the mammary areola, showing the orifices of the ducts of the glandular lobules.

Fig. 3. The mammary gland of the *Ornithorhynchus paradoxus* in a state of full development; the exact dimensions of the gland are preserved.

Fig. 4. The left ovary and Fallopian extremity of the oviduct of the same specimen. (The letters indicate the same parts as in Pl. XV.)

Fig. 5. *a.* A magnified view of a section of one of the lobules of the mammary gland, after having been injected with quicksilver, and dried.

b, b. The extremities of the ducts of the other lobules converging as they pass through the integument to the mammary areola.

c. The fibres of the panniculus carnosus.

d. The integument.

The preparations described in the preceding paper have been deposited in the Museum of the Royal College of Surgeons.

XXIII. *On the Water-Barometer erected in the Hall of the Royal Society. By* J. F. Daniell, *Esq. F.R.S. Professor of Chemistry in King's College, London.*

Read June 21, 1832.

I HAVE for some time entertained an opinion, in common with some others who have turned their attention to the subject, that a good series of observations with a Water-Barometer, accurately constructed, might throw some light upon several important points of physical science: amongst others, upon the tides of the atmosphere; the horary oscillations of the counterpoising column; the ascending and descending rate of its greater oscillations; and the tension of vapour at different atmospheric temperatures. I have sought in vain in various scientific works, and in the Transactions of Philosophical Societies, for the record of any such observations, or for a description of an instrument calculated to afford the required information with anything approaching to precision. In the first volume of the History of the French Academy of Sciences, a cursory reference is made, in the following words, to some experiments of M. Mariotte upon the subject, of which no particulars appear to have been preserved. "Le même M. Mariotte fit aussi à l'observatoire des experiences sur le baromètre ordinaire à mercure comparé au baromètre à eau. Dans l'un le mercure s'elèva à 28 pouces, et dans l'autre l'eau fut à 31 pieds $\frac{1}{3}$. Cequi donne le rapport du mercure à l'eau de $13\frac{1}{2}$ à 1." Histoire de l'Académie, tom. i. p. 234.

It also appears that Otto Guricke constructed a philosophical toy* for the amusement of himself and friends, upon the principle of the water-barometer;

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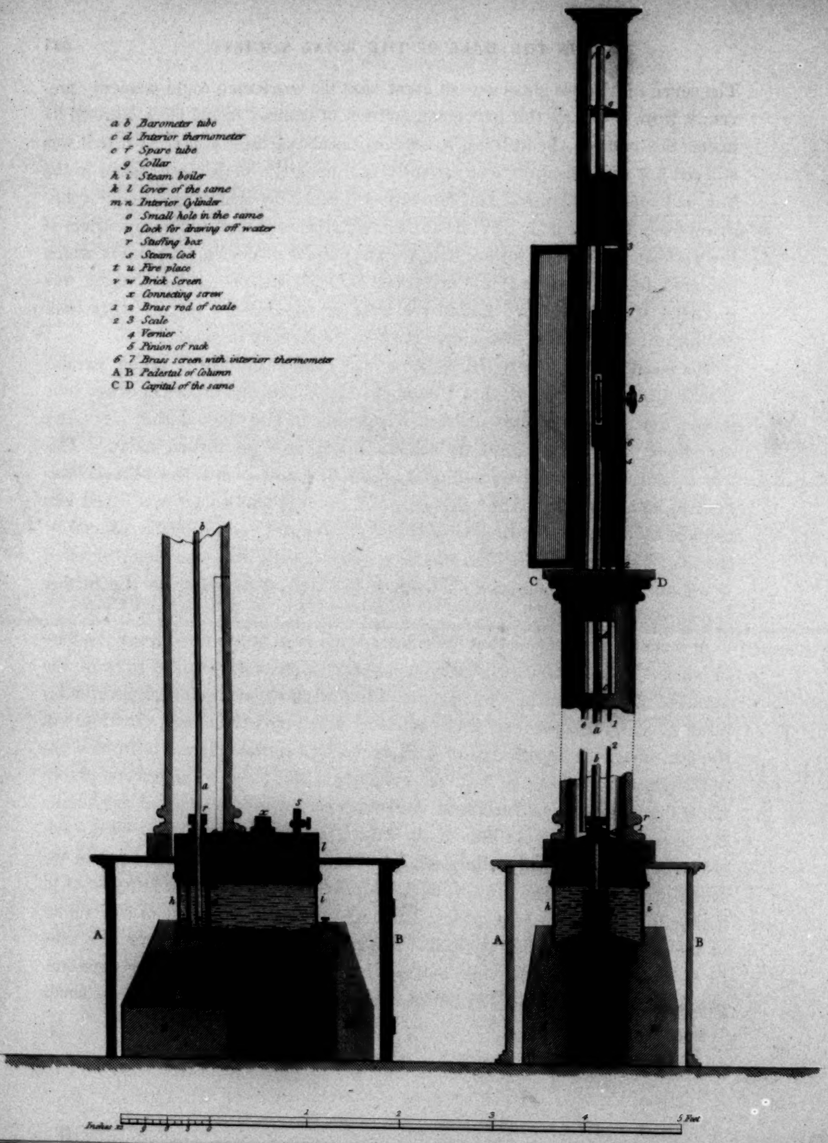
* It consisted of a tube above thirty feet, rising along the wall, and terminated by a tall and rather wide tube hermetically sealed, containing a toy of the shape of a man. The whole being filled with water and set in a bason on the ground, the column of liquid settled to the proper altitude, and left the toy floating on its surface; but all the lower part of the tube being concealed under the wainscoting, the little image made its appearance only in fine weather. To this whimsical contrivance he gave the name of *Anemoscope* or *Semper Vivum*.

but the column of water probably in this, as in all the other instances which I have met with, was raised by the imperfect rarefaction of the air in the tube above it, or by filling with water a metallic tube, of sufficient length, cemented to a glass one at its upper extremity, and fitted with a stop-cock at each end; so that when full the upper one might be closed and the lower opened, when the water would fall till it afforded an equipoise to the pressure of the atmosphere. The imperfections of such an instrument, it is quite clear, would render it totally unfit for the delicate investigations required in the present state of science; as, to render the observations of any value, it is absolutely necessary that the water should be thoroughly purged of air, by boiling, and its insinuation or reabsorption effectually guarded against. I was convinced that the only chance of securing these two necessary ends, was to form the whole length of tube of one piece of glass, and to boil the water in it, as is done with mercury in the common barometer. The practical difficulties which opposed themselves to such a construction long appeared to me insurmountable; but I at length contrived a plan for the purpose, which, having been honoured with the approval of the late Meteorological Committee of this Society, was ordered to be carried into execution by the President and Council.

The first object was to procure a glass tube of the proper diameter, and of sufficient length for the purpose. Messrs. PELLATT and Co., of the Falcon Glass House, very obligingly consented, upon application, to permit the trial to be made at their works; such an undertaking never having been before attempted. Accordingly, a very strong packing-case was prepared of one inch-and-a-half deal, forty feet long, five inches wide, and four inches deep, inside measure; with a cover of the same thickness to screw down upon it. This was carried to the glass-house, and being laid in the yard with its cover off, small pieces of wood were placed across its bottom, at about one-foot intervals. The only instructions given to the workman were to make a tube of the length of the box, which should not be less than half an inch internal diameter, and as equal throughout its length as possible; and the manual dexterity with which he proceeded to effect this was well worthy of admiration. Having collected the glass at the end of his tube, and blown the cavity, a boy attached another iron with a small lump of hot glass to the opposite extremity of the mass, and drew the tube out by walking away to the required distance.

a b *Barometer tube*
c d *Interior thermometer*
e f *Spare tube*
g *Collar*
h i *Steam boiler*
k l *Cover of the same*
m n *Interior Cylinder*
o *Small hole in the same*
p *Cock for drawing off water*
r *Stuffing box*
s *Steam Cock*
t u *Fire place*
v w *Brick Screen*
x *Connecting screw*
1 2 *Brass rod of scale*
2 3 *Scale*
4 *Vernier*
5 *Pinion of rack*
6 7 *Brass screen with interior thermometer*
A B *Pedestal of Column*
C D *Capital of the same*



C. S. Drewry del.

J. Basire sc.

The curve of the hot glass was so great that the workmen could scarcely prevent it from touching the pavement, (which of course would have caused its instant destruction,) by holding its extremities above their heads. While it was still red-hot and pliant, it was carefully laid upon the transverse pieces in the box, and rolled backwards and forwards till cool; by which a perfectly cylindrical form was secured. While the drawing process was going on, others of the workmen fanned with their hats, for the purpose of cooling, the parts which appeared to be extending too fast; and by such simple means a tube was perfected without a flaw, and of the greatest regularity; varying only from one inch diameter at its lower extremity to 0·8 inch at its upper.

The facility with which this process was conducted was so much greater than had been anticipated, that I immediately determined to have another tube made; that in case of any accident happening to the first, during the after operations, all the preliminary labour might not be thrown away. This was accordingly effected by rolling it upon the steps of a ladder placed horizontally upon the ground for that purpose. After it was cool it was lifted into the box by six men standing at equal intervals apart, and carefully placed by the side of the first. The box was then packed with hay, the cover screwed down, and carried upon men's shoulders to a convenient place for the further operations.

As it was not intended that the tubes should ever be removed from the case in which they had been originally deposited, the first step was to prepare the means of fixing them in their proper places when raised to the perpendicular position. For this purpose pieces of wood were provided of half the depth of the box, upon the upper edge of each of which a semicircle was hollowed out of the exact dimensions of half the cylinder of the tube. These were placed under the tube at equal intervals; and other similar pieces prepared for screwing down upon the upper side of the tube; in such a way that the two semicircles meeting, formed collars, which tightly embraced it, and fixed it in the centre of the box. The corners of the lower pieces were also cut away so as to inclose the spare tube (*e*, *f*), Plate XIX. which was placed in one of the angles of the case, and thus tightly fixed. The next object was to prepare the tube (*a*, *b*) itself for its final fixture; and for this purpose, as it was longer than was necessary, three feet were cut off from its upper extremity with a file; a small thermometer (*c*, *d*) which had been made for the purpose, with a platinum scale carrying a spring of the same metal upon its back, was pushed down into the tube to a situation where it had been calculated it would always be immersed in the water, notwithstanding its oscillations; and where a slight tapering of the tube insured its being fixed by the action of the spring. By a careful application of the blow-pipe the glass was now softened, and an external collar (*g*) pushed up upon it, about eight inches from its upper extremity. This was deemed necessary to give it additional support, and to prevent its slipping in its proper position. The upper extremity was then contracted and drawn out into a small tube six inches long and of about one quarter of an inch diameter. These preparations having been successfully completed, a small stop-cock was fitted to the upper end of the contracted tube by very careful grinding, and secured in its place by a little white lead. The tubes were then again packed in their case, and the cover screwed down.

A small copper steam-boiler (*h*, *i*) was now constructed of what is called the waggon shape, and which was intended to form the cistern of the barometer. Without the cylindrical cover (*k*, *l*) it is eighteen inches long, eleven inches wide, and ten inches deep. Its bottom is slightly arched; and towards one extremity on the inside is fixed a small cylinder (*m*, *n*) six inches high and three inches diameter; the object of which is to form a receptacle into which, the lower end of the tube being made to dip, the great body of the water might at any time be drawn out of the cistern, if required, without, for a short time, disturbing the water in the tube, or allowing any air to ascend into the vacuum. A small hole (*o*) was afterwards drilled in this cylinder, which is six inches from the crown of the arch, and four inches and a half from the bottom; so that the water might be more completely withdrawn. At the other extremity is a cock (*p*) for drawing it off, if at any time it should be necessary to change it. The cover (*k*, *l*) is an arch of the height of six inches. Immediately over the cylinder above described, a length of five inches (*k*, *q*) is fixed and fitted with a stuffing-box for the glass tube to pass through. Beyond this it is made to take off, but may be fixed down by means of screws: on the summit of this moveable end a cock (*s*) is placed. The whole of the interior has been strongly tinned.

Everything being now prepared, the steam-boiler was set with brick-work in a proper position over a small fire-place, with a temporary flue (*t*, *u*) at the foot of the well-staircase conducting to the apartments of the Society. With considerable difficulty and contrivance, the case with the glass tubes was introduced, by permission of the Antiquarian Society, through their library, and fixed against the stairs in a perpendicular direction, immediately over the stuffing-box; and the front of the box being removed, the tube was unpacked and suspended from above over the aperture. It was then very carefully lowered into its proper position in the boiler, and the wooden stays being screwed into their places, it was firmly adjusted. The stuffing-box (*m*, *n*), through which it passed into the boiler, was then packed with tow, and intended to be perfectly steam-tight. Part of the upper end of the deal-case was removed with a saw, so as to leave about six feet of the glass tubes exposed.

The object of the whole arrangement was as follows: first to boil the water in the cistern thoroughly, suffering the steam to escape by the cock (*s*), and then, by closing the latter, to raise the water in the tube, by the elastic force of the vapour acting upon its surface, till it issued in a jet from the small stop-cock upon its summit. When a sufficient current had thus been forced up, to secure the thorough wetting of the tube, and the total extrication of all particles of air, it was intended to close the stop-cock at the top while the water was still flowing, and at the same moment to relieve the pressure below by opening the cock upon the boiler, and again suffering the steam to escape. It was conceived that when the whole apparatus was cool, the column of water would subside, till it afforded a balance to the pressure of the atmosphere; when the small tube might be sealed by a dextrous application of the blow-pipe, and the stop-cock removed.

Everything being ready for the experiment, a preliminary trial was made of the apparatus on the 10th of June. The boiler was carefully washed with boiling distilled water, and the cover being screwed down, it was filled with distilled water to within five inches and a half of the top. The fire was then lighted in the grate, and in about two hours and a half a powerful current of pure steam issued from the cock (*s*). When this had continued for about half an hour, the cock was gradually closed, and the water rose very slowly in the tube. During its rise it oscillated backwards and forwards two or three inches, but the column was perfectly unbroken and clear. On this occasion it was found impossible to raise it higher than thirteen feet, owing to the stuffing-box and cover not being sufficiently close. The cock upon the boiler was therefore gradually opened, and the column of water slowly subsided, the steam rushing out with considerable violence. Several practical points were determined by this experiment, which it was of importance to be acquainted with. The apparatus was found perfectly manageable; the pressure could be regulated with great precision by the cock, and the elasticity of the steam increased by very slow degrees, even when quite shut off. The temperature of the rising column was very moderate, and felt but just warm to the hand at the upper part.

Several little alterations were made in the fire-place, and the part (*v*, *w*) which was immediately under the tube was bricked up, so that the flame was cut off from the front of the boiler, that the steam might be raised from the back part only, and the possibility of any bubble passing up into the tube precluded. The stuffing-box was repacked, and the top screwed down with greater care. The water was drawn off, and fresh distilled water poured in.

It was now determined to prove the apparatus, by raising the column of water by condensed air; and for this purpose the pump of a soda-water machine was connected, by means of a flexible pipe and screw, with a collar (*x*) fixed for the purpose upon the arch of the boiler. As the condensation proceeded, the column of water rose steadily, till it issued with considerable force from the aperture of a small glass tube fixed into the stop-cock on the summit, and bent to an angle to prevent the waste water trickling down the apparatus. When the force of the jet began to decrease, the stop-cock was closed, and the cock on the boiler at the same moment opened. After a short interval the column of water began slowly to decline, and appeared to boil violently from the extrication of air from its surface. This effervescence continued for more than an hour, with decreasing force; and the formation of air bubbles could be perceived nearly half way down the column. After eighteen hours, the water stood in the tube at about thirty feet eight inches from the level of the water in the cistern.

Advantage was taken of this opportunity to ascertain the relative capacities of the tube and cistern; and it was found, by careful measurement, that the fall of this quantity in the tube occasioned a rise in the level of that in the cistern of one inch and a half, affording a correction of very nearly 0·04 inch for ten inches. Everything having been thus prepared for the final experiment, a fire was lighted under the boiler at 11 A.M. of the 13th of June, and at half-past one pure steam issued with force from the cock (*s*) on the top of the boiler. When this was closed, the water began to rise slowly and steadily in the tube, oscillating at times about one inch and a half. More than an hour elapsed before the column of liquid reached the thermometer (*c*, *d*) at the upper end, when its temperature was found to vary from 85° to 90°. It still continued to rise very gently, till it issued with some force in an unbroken jet from the small tube which had been adjusted to the stop-cock. Three pints of water were thus drawn off, and the thermometer rose to 110°. The stop-cock on the top of the tube was then closed, and the cock on the top of the boiler simultaneously opened. The steam rushed forth from the latter with great violence, and after a considerable interval the column began very gently to fall from the top, without any boiling, or the slightest indication of air-bubbles. When it appeared to be stationary, the sealing was attempted; the small part of the tube, to which the stop-cock was attached, was successfully drawn off and closed without the slightest disturbance of the column of water; but in cooling it unfortunately cracked. The fissure thus occasioned was very minute, but rendered the resumption of the whole process necessary. The most difficult part of this to effect, was the drawing off and contraction of the tube to fit it again for sealing. It was determined, upon consideration, not to replace the stop-cock, but to rely upon the pressure of the operator's thumb to cut off the communication with the external air during the sealing.

As it was necessary to the operation that the tube should be turned upon its axis, it was unpacked from the stuffing-box of the boiler, and loosened from its different supports; and everything was again successfully adjusted with great dexterity by Mr. Newman, who overcame the difficulties of these various processes with the greatest skill. It would be tedious to repeat the further steps of the progress; the boiling was conducted precisely in the manner which I have just described, and the tube was finally and permanently closed on the 18th of June. Not the slightest speck or air-bubble has from that moment been detected in the column of water.

While the water in the boiler, which now constitutes the cistern of the barometer, was still warm, a quantity of the purest castor oil (Oleum Ricini), was poured into it till the surface was covered to the depth of half an inch; this was done for the purpose of cutting off the communication of the atmosphere with the water, and with the view of preventing the absorption of the air. Some of the same oil was poured upon the surface of some distilled water in a wide-mouthed glass vessel, and being lightly covered with paper was set by in a closet, that any change might be detected to which it might be liable under such circumstances.

The adjustment of a scale was the next object of importance. For this purpose a hollow brass rod (1, 2) was prepared of $\frac{3}{8}$ths of an inch diameter, and adjusted by means of a screw at the upper end to a flat ruler of brass (2, 3) divided into inches, and carrying a vernier (4) by which the hundredth part of an inch is easily read off, and which is moveable from the outside of the case of the instrument by means of a rack and screw (5). The same rack and screw also moves a brass screen (6, 7), which rises and falls with the vernier and protects the tube from the heating influence of the breath or hand; a small thermometer is inserted into this screen. The rod was measured from a scale formerly belonging to the late Mr. CAVENDISH, and now the property of Mr. NEWMAN, by marking it with a beam-compass at intervals of two feet, and afterwards repeating the process at intervals of sixteen inches. The two measures corresponded to the one twentieth of an inch; the difference being found to depend upon the multiplication of a small error in laying down the sixteen inches, and corrected accordingly.

The rod was next placed in the case of the barometer by the side of the tube, being made to pass through the wooden stays of the tube, in which it can freely move. At its lower end an ivory point of known length was fixed by which it was very carefully brought into exact contact with the surface of the oil in the cistern; the flat scale was then carefully adjusted to its upper end, and it was fixed at the lower end by screws to the top of the copper cistern. The column of water was thus found to stand exactly thirty-three feet four inches, or four hundred inches above the level of the fluid in the cistern. This, then, is the neutral point of the instrument, above or below which a correction of ±·02 inch must be made for every ascent or descent of five inches in the tube. The whole instrument has been inclosed in an exterior ornamental case resembling an architectural column. The pedestal (A, B) conceals the boiler with its brick-work, and upon the capital (C, D) stands a glass-case including that part of the tube to which the oscillations are confined, and the apparatus for measuring them.

As much interest will attach to the accurate comparison of the water-barometer with the mercurial barometer, it is of great importance that several corrections should be attended to in the first reading of their respective heights, to reduce the columns to the same invariable circumstances under which alone such comparison can be properly made; for this purpose the variations of the density of the liquids, and the expansion of the scales, from variations of temperature, together with the capillary action of the tubes, must be taken into account. To facilitate this object, I have constructed the two following Tables of double entry; by which the observations may be reduced to the temperature of 40° (39°·38) or that of the maximum density of water, in which the expansion of the brass scales is also allowed for; which is a correction of considerable amount in the long scale of the water-barometer.

The data upon which these Tables have been calculated are as follows:

1st, The specific gravity of water at different temperature, as determined by the experiments of Hallström, taken from Dr. Thomson's late work upon Heat and Electricity, p. 28.

2nd, The determination of the linear expansion of brass at ·0000104 per degree of Fahrenheit.

The height of the column is assumed to be in inverse proportion to the specific gravity; and the correction to the maximum density at 40° (or more correctly 39°·38) is calculated accordingly. From this correction is deducted, or to it is added, the expansion or contraction of the brass scale on either side of 60°, calculated on the preceding datum.

Table of Corrections for Temperature for the Water-Barometer. Standard Temperature of Scale 60°. Maximum Density of Water 40°.

Temperature.		Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
Exact.	Approx.	350	360	370	380	390	400	410
35·6	35	—·101	—·103	—·106	—·108	—·112	—·115	—·118
39·38	40	—·072	—·074	—·077	—·079	—·081	—·083	—·085
44·6	45	—·073	—·075	—·077	—·079	—·081	—·083	—·085
50	50	—·113	—·115	—·118	—·122	—·124	—·128	—·132
55·4	55	—·191	—·195	—·201	—·206	—·211	—·217	—·223
59	60	—·258	—·264	—·272	—·279	—·286	—·294	—·302
64·4	65	—·398	—·409	—·420	—·431	—·443	—·454	—·466
69·5	70	—·575	—·590	—·606	—·623	—·639	—·656	—·673
75·2	75	—·786	—·808	—·831	—·853	—·876	—·898	—·921

With regard to the capillary action of the tube, which of course is in the opposite direction to that of the mercurial barometer, Dr. YOUNG has calculated * that the central elevation for water in a tube of which the diameter is ·49964 inch (which is almost exactly the diameter of the tube within the range of the oscillations,) is ·035, and the marginal elevation ·172.

In my first use of the instrument I conceived that the observation was made with most certainty by bringing the vernier to coincide with the marginal elevation of the water; and in the following observations the correction of —·17 has been applied accordingly. Mr. HUDSON has since shown me, that by reflecting the light upon the column from behind, the observation from the centre is made with the greatest precision; and in some observations which have been kindly furnished by that gentleman, the correction of —·03 only has been applied. The difference of the two corrections deduced from the calculation of Dr. YOUNG as above, agrees very nearly with the difference of the two readings upon the barometer when carefully observed.

As the usual Tables for the thermometric correction of the mercurial barometer are calculated for 32°, I considered it necessary to calculate a fresh Table for the temperature of 40°; that both the water and the mercury might be reduced to the same standard temperature. The dilatation in volume of mercury per degree of FAHRENHEIT has been taken, on the authority of MM. DULONG and PETIT, at ·0001001 of the volume at 32°. And the height of the

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* YOUNG's Lectures on Natural Philosophy, vol. ii. p. 669.

column has been assumed to be in the ratio of the volume at 40° to the volume at the observed temperature. To the correction thus obtained has been added, or from it has been deducted, the expansion or contraction of the brass scale on either side of the standard temperature 60°.

Table of Corrections for Temperature for the Mercurial Barometer. Standard Temperature of Scale 60°. Volume of Mercury at 40° Standard.

Temperature.	Inches. 28·	Inches. 28·5	Inches. 29·	Inches. 29·5	Inches. 30·	Inches. 30·5
35	+·007	+·008	+·008	+·008	+·008	+·008
40	—·005	—·006	—·006	—·006	—·006	—·006
45	—·018	—·018	—·018	—·018	—·019	—·019
50	—·030	—·031	—·032	—·032	—·033	—·033
55	—·043	—·043	—·044	—·045	—·046	—·046
60	—·056	—·057	—·058	—·059	—·060	—·061
65	—·069	—·070	—·071	—·072	—·074	—·075
70	—·081	—·082	—·084	—·085	—·087	—·088
75	—·094	—·096	—·097	—·099	—·101	—·102

The mercurial barometer, with which the following comparison has been made, is of a portable construction, and has been fully described on a former occasion*. It is the first to which a platinum guard was ever applied, and it still remains perfectly free from air. The correction of +·044 for capillary action has been experimentally verified, upon more than one occasion, by comparison with a barometer of half an inch bore, in which no such correction is necessary.

I have not hitherto had it in my power to institute such a series of observations as I think the interest of the subject would have justified; as I have been obliged to depend upon my own exertions, or of those who from pure love of science have been willing to assist me in this laborious drudgery, at such intervals as the pressure of other engagements would permit. Of these by far the most important are the hourly observations of Mr. Hudson, which, with the assistance of some members of his family, he had the resolution to persevere in for fifteen days, and which he has communicated to the Society. Prior to these, were the following observations made at my request by Mr. Roberton in the months of August and September 1830, at different hours of the day;

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* Daniell's Meteorological Essays and Observations, 2nd edition.

but generally at 9 A.M. and 3 P.M. They include a very considerable range of temperature (from 57° to 74°), and serve to test the accuracy of the instruments brought into comparison shortly after the completion of the water-barometer, and that of the different corrections which have been applied to them.

The first column of the following Table records the date, and the second the hour of the observations. The third column contains the temperature of the internal thermometer (*c, d*), and the fourth that of the external thermometer (6, 7). The fifth shows the corrected height of the water-barometer; the sixth the temperature of the thermometer attached to the mercurial barometer. This, it will be observed, sometimes differs several degrees from the former; and, when this is the case, the mean has been taken as the temperature by which to correct the length of the scale; as standing at the bottom of the column, it most probably indicated the temperature of the lower extremity. The seventh column contains the corrected height of the mercurial barometer. In the eighth column I have placed the height of the column of water reduced to the corresponding height in mercury. As the basis of this calculation, I have taken the specific gravity of mercury at 40°, 13·624, as determined, at my request, by Mr. FARADAY at the time when I fitted up the large mercurial barometer belonging to the Society. The ninth column exhibits the differences of the two columns, or the amount of the depression of the column of water by the included vapour, expressed in parts of an inch of mercury.

By the side of these differences I have placed, in the tenth column, the elasticity of aqueous vapour due to the temperature of the surface water in the barometer, calculated from the data of Dr. URE. The eleventh column exhibits the differences of the two preceding. The mean results of every ten observations are also added to the register.

Register I.

Of the Temperature and Height of the Water and Mercurial Barometers.

1830.	Hour.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
		In.	Out.							
	A.M.	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
July 31	3	74·5	74·6	396·605	73·8	29·979	29·110	·869	·877	+·008
Aug. 1	9	67·3	67·7	398·111	67·2	29·927	29·221	·706	·699	+·007
–	10	68·0	68·3	397·728	67·8	29·924	29·192	·732	·722	+·010
–	3	70·5	70·7	396·327	71·7	29·879	29·090	·780	·770	+·010
2	12	66·2	66·6	396·158	65·8	29·772	29·077	·695	·678	+·017
3	9	63·6	63·7	399·243	63·6	29·943	29·304	·649	·615	+·034
4	3	68·7	68·7	397·661	69·3	29·921	29·188	·733	·733	+·000
5	2	69·6	70·2	396·413	69·7	29·869	29·097	·772	·770	+·002
27	1	61·5	61·8	395·025	64·2	29·636	28·994	·642	·594	+·048
28	9	58·2	58·6	391·755	58·2	29·337	28·754	·583	·526	+·057
Means..	..	66·8	67·1	396·503	67·1	29·809	29·103	·706	·699	+·007
Aug. 28	12	58·8	58·2	391·732	59·4	29·350	28·753	·597	·543	+·054
—	3	59·6	60·0	392·294	60·4	29·396	28·794	·602	·560	+·042
29	9	57·8	59·2	398·837	59·0	29·854	29·274	·580	·526	+·054
—	3	59·8	60·5	399·333	60·8	29·913	29·310	·603	·560	+·043
30	9	57·8	58·6	403·059	57·6	30·157	29·584	·573	·526	+·047
—	1	59·4	60·2	402·396	60·8	80·150	29·535	·615	·560	+·055
—	3	60·6	61·2	401·993	60·6	30·135	29·506	·629	·568	+·061
31	9	57·8	58·8	403·959	57·4	30·228	29·650	·578	·526	+·052
—	3	60·6	61·8	402·696	61·0	30·206	29·557	·649	·577	+·072
Sept. 1	9	58·8	59·2	404·417	58·5	30·273				
Means..	..	58·9	59·7	400·071	59·5	29·966	29·364	·602	·543	+·059
Sept. 1	3	62·0	63·0	402·886	63·2	30·244	29·571	·673	·605	+·068
2	9	57·8	58·4	402·742	56·0	30·149	29·560	·589	·526	+·069
–	3	61·0	62·0	400·246	63·0	30·033	29·377	·656	·594	+·062
–	6	61·8	62·0	399·186	63·0	29·974	29·300	·674	·594	+·080
3	9	58·2	58·5	397·739	58·2	29·837	29·192	·645	·526	+·119
–	3	60·0	60·6	396·952	61·4	29·771	29·136	·635	·560	+·075
4	9	58·5	59·4	399·277	58·2	29·890	29·296	·594	·534	+·060
–	3	60·2	60·8	398·895	60·4	29·959	29·278	·681	·560	+·121
5	9	57·5	58·0	396·239	56·2	29·672	29·083	·589	·526	+·063
–	3	60·6	60·8	395·293	61·3	29·656	29·014	·642	·568	+·074
Means..	..	59·7	60·3	398·945	60·0	29·918	29·282	·636	·560	+·074
Sept. 6	9	58·2	58·8	394·135	58·8	29·532	28·916	·616	·534	+·082
–	3	59·2	59·8	392·911	60·0	29·457	28·781	·676	·551	+·125
7	9	58·8	59·2	396·356	59·2	29·682	29·092	·590	·543	+·047
–	3	59·5	59·8	396·614	59·6	29·700	29·111	·589	·551	+·038
8	9	58·1	58·6	400·057	58·2	29·949	29·364	·585	·526	+·059
–	3	60·8	61·3	399·675	60·3	29·962	29·335	·627	·577	+·050
9	9	57·0	57·6	398·328	56·0	29·819	29·236	·583	·508	+·083
–	3	58·2	58·2	397·177	57·8	29·762	29·152	·610	·526	+·084
Means..		58·7	59·1	396·906	58·7	29·732	29·132	·600	·543	+·057

The most striking result of this comparison is, the almost exact coincidence in the first ten observations of the elasticity of the aqueous vapour, derived from the experiment, with the amount as determined from calculation in a range of temperature from 58° to 74°; the differences in the eleventh column being much less than I should have anticipated, even from the necessary uncertainty in ascertaining the temperature by the thermometers.

The remaining series exhibit larger and rather increasing differences, but such only as might fairly be supposed to come within the limits of errors of observation. It must also be observed that they were taken at greater intervals apart, a circumstance which I shall presently show may have had a considerable influence upon the results. The differences in the last column are, however, all, except the first, marked with the positive sign +, denoting that the depression from observation is invariably greater than that which would have resulted from the calculated elasticity of the vapour. This would rather indicate some constant error in some of the data of the calculation than the necessarily fluctuating errors of observation; and we should only have to assume the specific gravity of mercury as 13·590 instead of 13·624, and the mean difference would disappear. There can, therefore, I think, be no hesitation in coming to the conclusion that, considering the difficulty and complexity of the several adjustments, and the variety of the necessary corrections applied to the observations, the whole arrangement was even more perfect than could have been expected, up to the time of this first register.

It was a principal object with me, as soon as possible to obtain a good and uninterrupted series of observations during a long period, taken at least once a day at some fixed hour; and for this purpose I engaged a careful workman of Mr. NEWMAN's, who had been instructed in the reading of the different instruments, to keep a register of their indications at 7 A.M. in the summer months, and 7½ A.M. in the winter. By a careful comparison of his readings with those of others, he was found to be fully competent to the task. The following register contains these observations for one year and a half, commencing in October 1830, and ending in March 1832. They have been corrected in the same way as the last, and the same kind of comparison instituted. The depression of the water-barometer has been worked out daily for the first two and the last months; but for the intermediate months I have satisfied myself with making the calculation for the monthly mean results.

The gradually increasing differences between this depression and the elasticity due to the vapour, have forced upon my mind the unwelcome conviction that, by some means or other, gaseous matter has crept into the instrument; and under this impression it was useless to carry the calculations further.

REGISTER II.

Temperature and Height of the Water and Mercurial Barometers at 7 A.M. in the Summer, and 7^{h} 30^{m} A.M. in the Winter, from October 1830 to March 1832.

1830.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
October 9	56	56	406·48	55	30·416	29·836	·580	·492	·088
10	56	56	406·85	56	30·438	29·863	·575	·492	·083
11	55·5	55	405·63	50	30·369	29·773	·596	·484	·112
12	55·5	56	404·50	53	30·231	29·690	·541	·484	·057
13	55·5	55·5	405·04	52	30·329	29·730	·599	·484	·115
14	56	56	404·50	52	30·252	29·690	·562	·492	·070
15	55	54·5	403·46	50	30·215	29·614	·601	·476	·125
16	55	54	403·93	45	30·166	29·649	·517	·476	·041
17	54	54	405·08	47	30·322	29·733	·589	·460	·129
18	53·5	53	404·52	48	30·220	29·692	·528	·452	·076
19	55	55	400·50	54·5	29·905	29·397	·508	·476	·032
20	57	57	399·89	59	29·982	29·352	·630	·508	·122
21	58	59	401·38	59	30·124	29·461	·663	·526	·137
22	61	61	402·29	62	30·279	29·528	·751	·577	·174
23	61	60·5	403·56	58	30·310	29·621	·689	·577	·112
24	57	57	405·60	52·5	30·411	29·771	·640	·508	·132
25	55	55·5	401·34	55	30·080	29·458	·622	·476	·146
26	57	56·5	399·28	52	29·897	29·307	·590	·508	·082
27	50·5	50	405·97	41	30·348	29·798	·550	·407	·143
28	53	53	399·72	54	29·938	29·339	·599	·444	·155
29	56	55	394·85	55	29·655	28·982	·673	·492	·181
30	54·5	54	399·93	52·5	29·945	29·354	·591	·460	·131
31	54	53·5	399·32	54	29·901	29·310	·591	·460	·131
Means....	55·7	55·5	402·77	52·8	30·162	29·563	·599	·476	·123

1830.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
Nov. 1	56	56	401·18	57	30·066	29·446	·620	·492	·128
2	57	56·5	401·38	57	30·095	29·461	·634	·508	·126
3	56·5	56·5	398·75	57·5	30·009	29·268	·741	·500	·241
4	57	57	397·07	56	29·773	29·145	·628	·508	·120
5	56	55·5	399·11	56·5	29·897	29·294	·603	·492	·111
6	57	57	393·90	58	29·574	28·912	·662	·508	·154
7	57·5	57	388·51	58	29·093	28·516	·577	·517	·060
8	57	57	394·79	54·5	29·602	28·978	·624	·508	·116
9	55	54	398·74	52	29·855	29·266	·589	·468	·121
10	53	53	397·55	53	29·772	29·180	·592	·444	·148
11	55·5	55	394·61	55	29·498	28·964	·534	·476	·058
12	53	53	399·61	44·5	29·913	29·332	·581	·444	·137
13	51	51	399·24	52·5	29·863	29·304	·559	·414	·145
14	55	55	394·90	54·5	29·574	28·985	·589	·476	·113
15	54·5	54	394·60	54	29·492	28·964	·528	·460	·068
16	55·5	55	391·38	55	29·347	28·727	·620	·476	·144
17	55	55	392·66	54	29·420	28·821	·599	·476	·123
18	51	51	397·43	54	29·708	29·171	·537	·414	·123
19	52	52·5	402·97	50	30·156	29·578	·578	·428	·150
20	50·5	50	401·41	51	30·022	29·463	·559	·400	·159
21	50	49	400·01	52	29·904	29·367	·537	·394	·143
22	54	53	396·61	54	29·596	29·111	·485	·468	·017
23	53	53	402·18	52	30·115	29·520	·595	·444	·151
24	50·5	51	406·07	50	30·380	29·805	·575	·407	·168
25	47·5	47	406·68	47	30·368	29·850	·518	·364	·154
26	49	49	403·23	47·5	30·153	29·597	·556	·388	·168
27	48·5	49	398·00	49	29·760	29·213	·547	·388	·159
28	46·5	46	394·91	48·5	29·485	28·986	·499	·352	·147
29	49	48	398·15	50	29·741	29·223	·518	·382	·136
30	49	49	399·71	50	29·884	29·339	·545	·388	·157
Means....	53·1	52·8	398·18	52·8	29·770	29·226	·544	·444	·100

1830.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
Dec. 1	50·5	50	401·04	50	29·993				
2	48	48	399·52	49	29·857				
3	49	48	396·06	49	29·505				
4	47·5	47	397·97	48	29·717				
5	47·5	47	398·33	46	29·784				
6	47	47	389·76	49	29·121				
7	50	49	388·73	50	29·118				
8	55	55	390·13	50	29·175				
9	48	48	386·57	50	28·927				
10	51	51	387·49	50	28·992				
11	45·5	46	392·29	47	29·286				
12	43	43	393·99	47	29·374				
13	40	39·5	404·81	44	30·170				
14	43	43	407·13	46	30·394				
15	44	43	408·03	48	30·526				
16	43	43	406·57	48	30·334				
17	40·5	40	403·23	45	30·066				
18	40	40	404·37	43	30·172				
19	41·5	41	405·12	45	30·208				
20	43	43	395·23	47	29·475				
21	45	45	395·67	48·5	29·525				
22	47·5	47	396·11	50	29·576				
23	42	42	394·44	48	29·415				
24	34·5	36	392·90	41	29·455				
25	37	37	393·50	34	29·311				
26	36·5	36	393·95	40	29·347				
27	39	39	392·21	42	29·806				
28	40	40	389·81	44	29·048				
29	42	42	397·53	43	29·665				
30	42·5	43	395·13	45	29·488				
31	45	45	390.59	49	29·158				
Means....	44·1	44	396·39	46·3	29·613	29·094	·519	·328	·191

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
January 1	46	45	398·23	46	29·743				
2	46	46	399·31	50	29·823				
3	46	46	399·73	49	29·872				
4	47	47	399·29	46	29·862				
5	45	45	399·21	49	29·820				
6	47	47	403·73	47	30·170				
7	44·5	44	409·19	45	30·578				
8	41·5	41	409·79	44	30·604				
9	42	42	405·45	46	30·269				
10	45	45	400·12	48	29·882				
11	44	44	402·56	48	30·156				
12	45	44·5	402·64	47	30·078				
13	44	44	403·15	44	30·088				
14	44·5	44	403·63	48	30·157				
15	42·5	42	401·93	46	29·968				
16	41	41	399·75	45	29·832				
17	43·5	43	396·63	46	29·518				
18	45	45	395·43	46	29·548				
19	48	48	396·30	50	29·617				
20	46·5	46	393·65	50	29·425				
21	48	48	389·21	47	29·117				
22	50	50	389·94	52	29·187				
23	50	50	390·93	53	29·274				
24	46·5	46	394·44	49	29·455				
25	45·5	45	398·27	47	29·756				
26	39·5	39	403·22	44	30·069				
27	43	43	401·89	41	30·049				
28	43·5	43	396·85	46	29·530				
29	42	42	400·17	45	29·863				
30	41·5	41	399·75	45	29·830				
31	42	42	398·53	45	29·753				
Means....	44·7	44·5	399·45	46·9	29·835	29·319	·516	·340	·176

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
February 1	42	41·5	390·61	44	29·177				
2	42	42	390·08	44	29·188				
3	41	41	394·31	44	29·431				
4	45·5	45	388·04	48	29·031				
5	43	43	396·29	47	29·580				
6	43·5	43	400·59	46	29·931				
7	45·5	45	394·46	48	29·490				
8	49	49	398·68	52	29·849				
9	51·5	51	399·35	53	29·723				
10	53	53	402·21	54	30·169				
11	55	55	402·07	55	30·196				
12	53	53	402·82	56	30·230				
13	53·5	53	402·47	56	30·212				
14	54	54	402·17	53	30·169				
15	53	53·5	401·18	50	30·056				
16	52	52	397·86	51	29·825				
17	50·5	50	398·95	52	29·876				
18	48	47	403·08	51	30·172				
19	48	48	401·28	51	30·050				
20	48	47·5	399·87	47	29·935				
21	45	44·5	401·63	43	30·045				
22	43	43	401·17	47	29·995				
23	44·5	44	406·16	42·5	30·389				
24	45·5	45	403·14	47	30·158				
25	49	48	397·98	50	29·820				
26	47	47·5	391·20	50	29·209				
27	46·5	46	391·74	49	29·325				
28	46	46	394·59	46	29·526				
Means....	47·8	47·5	398·35	49·2	29·813	29·239	·574	·376	·198

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
March 1	44	43·5	398·94	48	29·810				
2	46·5	46	397·56	50	29·763				
3	51·5	51	394·41	53	29·582				
4	53	52·5	395·64	54	29·676				
5	52·5	52	397·23	55	29·818				
6	53	53	389·37	55	29·167				
7	52	51·5	384·48	54	29·581				
8	49	48·5	397·12	53	29·773				
9	50	50	394·19	53	29·528				
10	48	48	398·83	50	29·871				
11	53·5	53	396·05	54	29·724				
12	49·5	49	398·94	53	29·907				
13	50·5	50	394·99	52	29·632				
14	50	49·5	395·42	52	29·644				
15	48·5	48	397·20	51	29·767				
16	52	52	394·50	53	29·566				
17	54·5	54	396·88	56	29·816				
18	54·5	54	401·01	55	30·130				
19	49	49	402·48	51	30·194				
20	50	50·5	400·92	45	30·037				
21	52·5	52	400·18	53	30·044				
22	51·5	51·5	402·42	47	30·199				
23	48	48	404·55	49	30·347				
24	44·5	44	401·02	47	30·031				
25	44	44	396·11	47	29·656				
26	46	46	391·73	48	29·342				
27	49·5	49	397·82	51	29·838				
28	51	51	399·74	48	29·994				
29	50	50·5	401·41	45	30·127				
30	48	48	403·08	43	30·232				
31	47	47	404·82	48	30·352				
Means....	49·8	49·5	397·71	50·8	29·843	29·191	·652	·400	·252

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
April 1	47·5	47	404·81	50	30·422				
2	45·5	45	401·11	49	30·045				
3	46	46	399·65	49	29·944				
4	46·5	46	396·43	50	29·604				
5	45·5	45	393·76	49	29·484				
6	49	49	392·98	50	29·462				
7	49	49	391·51	47	29·382				
8	52	52	388·58	54	29·220				
9	51	51	393·19	54	29·434				
10	53	53	393·14	55	29·530				
11	54	54	397·99	55	29·909				
12	54	54	397·36	53	29·883				
13	56	56	390·32	53	29·883				
14	57	57	389·75	54	29·873				
15	54·5	54	398·48	55	29·971				
16	56	56	390·50	54	30·003				
17	56	56	389·81	53	29·933				
18	52	52	399·43	53	30·016				
19	52·5	52	398·12	48	29·924				
20	52	52	396·51	51	29·697				
21	53	53	392·91	49	29·544				
22	54	54	390·97	52	29·407				
23	56	56	391·69	52	29·470				
24	55·5	55	395·42	53	29·762				
25	55	55	397·95	53	29·943				
26	55	55	395·82	52	29·795				
27	56	56	393·48	53	29·620				
28	55·5	55	390·49	52	29·324				
29	55	55	397·83	51	29·185				
30	56	56	390·84	56	29·397				
Means....	52·7	52·5	394·69	51·9	29·702	28·970	·732	·432	·300

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
May 1	55	55	390·86	54	29·400				
2	56	56	391·74	52	29·479				
3	55·5	55	393·00	53	29·568				
4	56	56	389·84	57	29·587				
5	54	54	392·22	48	29·484				
6	51	51	395·38	52	29·685				
7	48·5	49	399·25	51	29·974				
8	48·5	49	402·21	52	30·203				
9	50	50	433·55	52	30·285				
10	52	52	401·01	54	30·163				
11	51	51	400·00	53	30·136				
12	54	54	399·95	55	30·100				
13	55	55	397·28	52	29·919				
14	54	54	399·01	48	30·026				
15	51·5	52	398·93	47	30·001				
16	54	54	399·43	50	30·064				
17	56	56	400·05	55	30·153				
18	58	58	397·53	57	29·994				
19	58·5	58	394·23	57	29·753				
20	61	61	393·49	61	29·639				
21	60·5	60	394·90	59	29·827				
22	59·5	60	396·66	58	29·950				
23	59	59	395·19	57	29·846				
24	61	61	393·88	60	29·785				
2 5	62	62	393·77	61	29·771				
26	61	61	394·35	58	29·802				
27	59	59	393·66	55	29·736				
28	58·5	59	395·44	57	29·841				
29	58	58·5	396·31	56	29·913				
30	57	57	395·69	55	29·838				
31	57	57	396·99	55	29·930				
Means....	55·9	55·9	397·28	54·5	29·866	29·161	·705	·492	·213

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
June 1	57	57	395·59	57	29·843				
2	58	58	398·21	55	30·063				
3	58	58	398·54	55	30·100				
4	59	59	390·52	56	30·130				
5	60	60	397·47	59	30·058				
6	59	59	390·01	56	29·900				
7	58	58	390·30	55	30·010				
8	57	57	394·58	55	29·787				
9	58	57·5	394·47	56	29·795				
10	60	60	392·25	59	29·642				
11	60	60	390·64	60	29·539				
12	62	62	392·31	62	29·709				
13	62	62	393·72	61	29·791				
14	62·5	62	397·07	61	30·076				
15	62	62	394·76	62	29·827				
16	61	61	393·50	60	29·775				
17	61	61	394·48	59	29·864				
18	61	61·5	395·65	60	29·946				
19	63	63	394·08	63	29·848				
20	61·5	61	397·53	59	30·101				
21	62	62	397·63	60	30·129				
22	63	62·5	397·32	61	30·128				
23	64	64	397·11	66	30·129				
24	63·5	63	394·51	59	29·915				
25	61·5	61·5	392·59	60	29·718				
26	59·5	60	390·85	58	29·568				
27	59	59	394·60	57	29·837				
28	60	60	393·94	59	29·814				
29	59·5	59·5	395·83	58	29·954				
30	59·5	59·5	395·49	58	29·926				
Means....	60·4	60·3	394·52	58·9	29·897	28·952	·945	·560	·385

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
July 1	60	60	396·67	59	30·030				
2	61	61	395·55	59	29·959				
3	62	62	394·97	61	29·935				
4	62·5	62·5	397·17	61	30·126				
5	64	64	397·06	63	30·165				
6	65	64·5	398·21	64	30·259				
7	64·5	64·5	398·23	63	30·269				
8	64	64	397·16	62	30·167				
9	65	64·5	396·03	63	30·093				
10	68	68	393·38	67	29·977				
11	63·5	63·5	392·76	62	29·832				
12	65	65	389·30	64	29·586				
13	64	64·5	389·02	63·5	29·627				
14	63	62·5	390·74	63	29·668				
15	62	62·5	391·02	62	29·679				
16	62·5	62·5	391·23	63	29·697				
17	62	62·5	393·50	63	29·878				
18	62·5	62	394·58	62·5	29·956				
19	62·5	62	393·18	62·5	29·860				
20	63	62·5	391·65	62·5	29·750				
21	64·5	64	389·78	65	29·626				
22	62	61·5	392·13	63	29·756				
23	62·5	62	392·10	63	29·768				
24	61·5	61·5	391·80	60	29·730				
25	63	62·5	394·73	61	29·985				
26	63	62·5	396·11	61·5	30·097				
27	65	64·5	396·30	63	30·166				
28	67·5	67	394·68	66	30·097				
29	67·5	67	394·02	66	30·034				
30									
31									
Means....	63·5	63·3	393·90	62·7	29·923	28·912	1·011	·636	·475

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
August 1									
2	67	67	391·40	65·5	29·831				
3	67·5	68	390·84	67	29·818				
4	67	67	390·32	66	29·756				
5	68·5	69	398·14	68	29·603				
6	67	66·5	399·34	64	29·655				
7	66	66·5	398·75	66	29·603				
8	66	66	390·45	64	29·739				
9	68	68	391·54	67	29·870				
10	68	68	392·50	65·5	29·950				
11	67	67	394·74	64·5	30·072				
12	66	66	394·64	63	30·083				
13	66·5	67	393·30	64·5	30·003				
14	66	66	392·89	63	29·946				
15	65·5	65	393·95	62	30·026				
16	66	66	394·10	63	30·061				
17	65	65·5	393·46	64	29·992				
18	64·5	64	392·67	60	29·895				
19	62	62	389·78	61	29·646				
20	62	62	388·25	60·5	29·516				
21	63·5	64	393·94	63·5	29·986				
22	63	62·5	396·87	61	30·266				
23	64·5	64·5	395·26	63·5	30·125				
24	65·5	65	392·66	63	29·925				
25	64·5	64·5	389·00	63	29·618				
26	62	62	393·03	59·5	29·885				
27	63·5	64	392·45	64	29·889				
28	65	64·5	393·91	60·5	30·004				
29	63	62·5	395·89	59·5	30·141				
30	64·5	64·5	394·55	62	29·876				
31	66	66	391·42	66	29·880				
Means....	65·3	65·3	393·33	62·0	29·889	28·870	1·019	·657	·462

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
			Inches.		Inches.	Inches.	Inch.	Inch.	Inch.
Sept. 1	63°	62°·5	392·65	60°	29·887				
2	59	59	392·13	54	29·767				
3	58	58	393·51	53	29·852				
4	58	58	392·56	54	29·786				
5	62	62	392·11	63	29·721				
6	64	63·5	391·94	64	29·846				
7	61	60·5	392·48	56	29·835				
8	58·5	58	390·97	54	29·656				
9	57	57	389·34	53	29·541				
10	57·5	58	392·14	66·5	29·720				
11	57	57·5	394·18	56	29·908				
12	58·5	59	396·36	59	30·101				
13	59	59·5	396·79	57·5	30·159				
14	59·5	60	395·80	59	30·071				
15	59	59	396·24	56	30·100				
16	59	59	396·76	59	30·150				
17	59	59·5	397·09	58	30·190				
18	59	59	395·54	58	30·069				
19	59·5	59	397·32	58	29·884				
20	57·5	58	392·70	52	29·798				
21	58·5	59	391·64	59	29·745				
22	59	59	393·04	56·5	29·857				
23	58	57·5	395·68	55	30·029				
24	59·5	59·5	396·70	59·5	30·152				
25	60	60	394·59	59	30·001				
26	61	61	393·83	61	29·958				
27	61	61	392·83	60	29·855				
28	62	61·5	390·77	60	29·680				
29	62·5	62·5	388·51	62	29·577				
30	63	63	387·34	62·5	29·502				
Means....	59·6	59·7	393·45	58·1	29·880	28·879	1·001	·560	·441

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
October 1	63·5	63·5	384·11	63·5	29·236				
2	63	62·5	384·94	62	29·269				
3	62	62	389·45	60	29·610				
4	62	62	390·52	61	29·689				
5	61·5	61	392·07	59	29·805				
6	59·5	59·5	393·23	59	29·874				
7	62	61·5	390·60	63	29·711				
8	63	62·5	389·55	61·5	29·658				
9	61	60·5	389·73	57	29·613				
10	59·5	60	399·31	59	29·558				
11	61	61·5	389·92	61	29·629				
12	60	60·5	390·61	61	29·694				
13	60·5	60·5	390·00	60	29·641				
14	63	62·5	388·20	64	29·538				
15	62	62	390·61	63	29·605				
16	60	60·5	395·63	60	30·091				
17	58·5	58	398·59	54·5	30·288				
18	60	60	398·98	59	30·354				
19	60·5	60	397·06	59	30·213				
20	61	60·5	393·84	59	29·937				
21	60	60	392·94	58	29·866				
22	57	57	395·53	52	30·007				
23	59	59	393·77	60	29·918				
24	59	59	395·23	57·5	30·031				
25	57·5	58	393·34	56·5	29·878				
26	59	59	388·33	59·5	29·477				
27	58·5	59	390·75	58·5	29·672				
28	58	58	394·47	57	29·946				
29	57·5	58	398·09	57·5	30·260				
30	56·5	56·5	398·53	52·5	30·265				
31	56·5	56·5	397·22	56	30·165				
Means....	60	60	392·75	59	29·824	28·827	·997	·560	·437

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
Nov. 1	57	57	395·11	57	30·002				
2	57	57	391·77	58	29·722				
3	51·5	51	389·52	54	29·473				
4	49·5	49	392·97	50	29·696				
5	51	50·5	390·70	51·5	29·557				
6	51·5	51·5	391·43	51	29·616				
7	53	52·5	388·99	52	29·431				
8	51·5	51	391·36	52·5	29·601				
9	52	51·5	397·31	50·5	30·086				
10	51	51	401·12	45	30·416				
11	50	50	398·83	52	30·212				
12	53·5	53	399·17	54	30·278				
13	52·5	52·5	395·39	50·5	29·963				
14	50	50	392·11	48	30·064				
15	50	49·5	390·05	49·5	29·476				
16	44	44	388·37	46·5	29·283				
17	43	43	392·17	45	29·539				
18	42	42·5	393·49	43	29·669				
19	43	43	390·26	45	29·420				
20	42·5	42·5	394·93	45	29·761				
21	48	48	392·17	48·5	29·621				
22	51·5	51	393·77	52·5	29·807				
23	54	53·5	394·86	55	29·932				
24	54·5	55	395·30	55	29·973				
25	55	55	394·51	56	29·931				
26	56	56	393·74	56·5	29·888				
27	54	54	398·74	53	30·245				
28	48·5	48	402·41	48	30·468				
29	46·5	46	403·07	50	30·522				
30	46	45·5	400·95	45·5	30·343				
Means....	50·3	50·1	394·49	50·7	29·866	28·955	·911	·400	·511

1831.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
Dec. 1	48	48	398·17	50·5	30·135				
2	50·5	50	396·35	52	30·021				
3	52	51·5	397·29	52·5	30·120				
4	51	50·5	396·25	51·5	30·013				
5	51·5	51·5	393·73	52·5	29·814				
6	52	52	388·33	53	29·385				
7	53	53	382·21	54·5	28·962				
8	54	54	384·41	55·5	29·124				
9	56	56	384·33	57·5	29·139				
10	55·5	55	387·54	57	29·366				
11	56	56	386·65	57·5	29·315				
12	56·5	56	386·64	56	29·308				
13	56·5	56	386·66	57	29·306				
14	56	56	389·44	55·5	29·539				
15	55	55	382·29	51	29·740				
16	54	54	383·35	50	29·820				
17	52·5	52	391·95	52	29·674				
18	53	53	387·43	54	29·330				
19	51	50·5	390·15	50	29·507				
20	51	51	393·05	53·5	29·760				
21	52	52	391·01	53	29·623				
22	49·5	49	394·33	48	29·853				
23	49	49	393·42	48	29·765				
24	47	46·5	399·04	50	30·196				
25	44	43·5	400·99	41	30·346				
26	43	43	400·30	46	30·261				
27	44	44	401·82	45	30·435				
28	44·5	44	401·85	46	30·416				
29	46	45·5	400·46	47	30·320				
30	46·5	46	399·15	46	30·210				
31	45·5	45	399·43	44·5	30·221				
Means....	50·5	50·6	392·51	51·2	29·775	28·786	·989	·414	·575

1832.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
			Inches.		Inches.	Inches.	Inch.	Inch.	Inch.
January 1	44°	44°	399·60	41°	30·236				
2	44·5	44	399·61	43	29·977				
3	43	43	394·88	45	29·822				
4	44	43·5	393·58	43	29·531				
5	42·5	42	392·88	41	29·661				
6	43·5	43	391·45	46	29·558				
7	45	45	389·03	46·5	29·404				
8	45·5	45	389·14	49	29·383				
9	46	46	390·37	49	29·484				
10	48·5	48	391·17	50	29·566				
11	50	50	393·33	53	29·781				
12	50	49·5	393·63	52	29·801				
13	50	50	393·62	49	29·574				
14	48	48	397·77	45	30·119				
15	46·5	46	401·79	44	30·436				
16	45	45	402·23	47	30·467				
17	47	47	400·43	47	30·333				
18	48·5	48	400·21	51	30·337				
19	48	48	400·42	48	30·332				
20	45	45	398·77	41	30·173				
21	46·5	46	398·64	46·5	30·178				
22	48	48	399·08	49	30·235				
23	48·5	48	399·65	47	30·284				
24	47	47	399·18	49	30·225				
25	49	49	393·79	50	29·808				
26	48	48	394·18	48·5	29·844				
27	49	48·5	395·10	46	29·905				
28	45·5	45	399·47	43	30·235				
29	47	47	398·53	48	30·184				
30	48	48	399·93	48	30·317				
31	49·5	49	397·03	50	30·146				
Means....	46·8	46·6	396·38	47	29·979	29·094	·885	·364	·521

1832.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
February 1	48	48	390·22	47	29·523				
2	50·5	50	386·22	51	29·218				
3	50	49·5	389·46	46	29·459				
4	50·5	50	393·41	51	29·797				
5	52	52	395·35	53·5	29·983				
6	53·5	53	393·10	54·5	29·816				
7	52	52	394·16	50	29·888				
8	49·5	49	399·63	52	30·301				
9	51	51	400·04	52	30·364				
10	50	49·5	402·41	50	30·542				
11	50	49·5	400·21	49	30·350				
12	48	47·5	397·94	51·5	30·154				
13	48	47·5	397·18	49	30·073				
14	47	47	396·80	50·5	30·054				
15	46	44·5	397·37	48	30·060				
16	43	42·5	395·17	41·5	29·867				
17	45	45	393·02	48	29·708				
18	46	46	398·35	49	30·165				
19	47	46·5	400·02	50	30·307				
20	46	46·5	400·11	46	30·325				
21	46	45·5	399·43	48	30·263				
22	44	44·5	400·42	49	30·324				
23	44	44	400·05	43	30·380				
24	43	43	398·89	46	30·168				
25	42	42	397·21	43	30·019				
26	43	42·5	399·22	43·5	30·213				
27	46	46	397·72	44	30·112				
28	44·5	44	398·80	48	30·104				
29	44	44	398·91	48	30·193				
Means....	47·2	47	396·92	48·3	30·060	29·133	·927	·364	·563

1832.	Thermometers.		Water-Barometer.	Temperature of Mercury.	Mercurial Barometer.	Water-Barometer reduced to Mercury.	Difference.	Elasticity of Vapour.	Difference.
	In.	Out.							
	°	°	Inches.	°	Inches.	Inches.	Inch.	Inch.	Inch.
March 1	44	44·5	399·85	46	30·280	29·349	·931	·328	+·603
2	47	47	399·83	49	30·299	29·348	·951	·364	+·587
3	46	46·5	399·82	47	30·305	29·346	·959	·352	+·607
4	48	47·5	396·26	48	30·018	29·085	·933	·376	+·557
5	48·5	48	393·23	49·5	29·770	28·864	·906	·376	+·530
6	47	46·5	394·16	49	29·829	28·916	·913	·364	+·549
7	48	48	388·58	49	29·391	28·521	·870	·376	+·494
8	45	45	390·14	45	29·486	28·636	·850	·340	+·510
9	44·5	44	396·73	45	30·008	29·120	·888	·328	+·560
10	45	45	401·05	48	30·377	29·437	·940	·340	+·600
11	44	44	398·98	43·5	30·205	29·285	·920	·328	+·592
12	44	44·5	397·03	48	30·042	29·142	·900	·328	+·572
13	45	45	394·79	48	29·920	28·978	·942	·340	+·602
14	47	46·5	390·16	49	29·507	28·637	·870	·364	+·506
15	46·5	46	388·45	49·5	29·360	28·512	·848	·352	+·496
16	47·5	47	393·83	43	29·816	28·907	·909	·364	+·545
17	50	49·5	388·53	50·5	29·415	28·518	·897	·400	+·497
18	49	48·5	389·91	49	29·503	28·619	·884	·388	+·496
19	48	48	394·17	47·5	29·847	28·932	·915	·376	+·539
20	50	50	389·41	50	29·492	28·583	·909	·400	+·509
21	50	50	395·59	51·5	29·995	29·036	·959	·400	+·559
22	51	50·5	397·12	52·5	30·128	29·148	·980	·414	+·566
23	52	52	394·76	54	29·961	28·975	·986	·428	+·558
24	52	51·5	393·15	47·5	29·818	28·857	·961	·428	+·533
25	47·5	47·5	397·31	50·5	30·117	29·162	·955	·364	+·591
26	48	48	397·86	51	30·164	29·203	·961	·376	+·585
27	50	50	395·87	47·5	30·010	29·057	·953	·400	+·553
28	48	48·5	396·94	52	30·090	29·135	·955	·376	+·579
29	49	49	395·68	47·5	29·989	29·042	·947	·388	+·559
30	49·5	49	394·89	51·5	29·972	28·985	·987	·388	+·599
31	50	50	393·31	52	29·824	28·869	·955	·400	+·555
Means....	47·8	47·6	394·75	48·7	29·901	28·974	·927	·376	+·551

It will be observed how very gradually the differences, recorded in the last columns of the months, increase; till, in the month of March 1832, they average ·551; more than half an inch of mercury, indicating a mean depression of the water-barometer of more than seven inches. This result is further confirmed by a comparison of the monthly mean heights of the two instruments, and by observing that in the month of March 1832, when the differences for each day are exhibited, the greatest differences occur with the highest barometer, as would happen from the greater compression of included air under such circumstances. The regularity of this secondary effect is indeed very remarkable.

This unfortunate result not being doubtful, I determined to open the boiler for the purpose of throwing some light, if possible, upon the cause. Dr. Prout, to whose valuable advice I have been greatly indebted in all the previous arrangements, did me the favour of assisting at this examination.

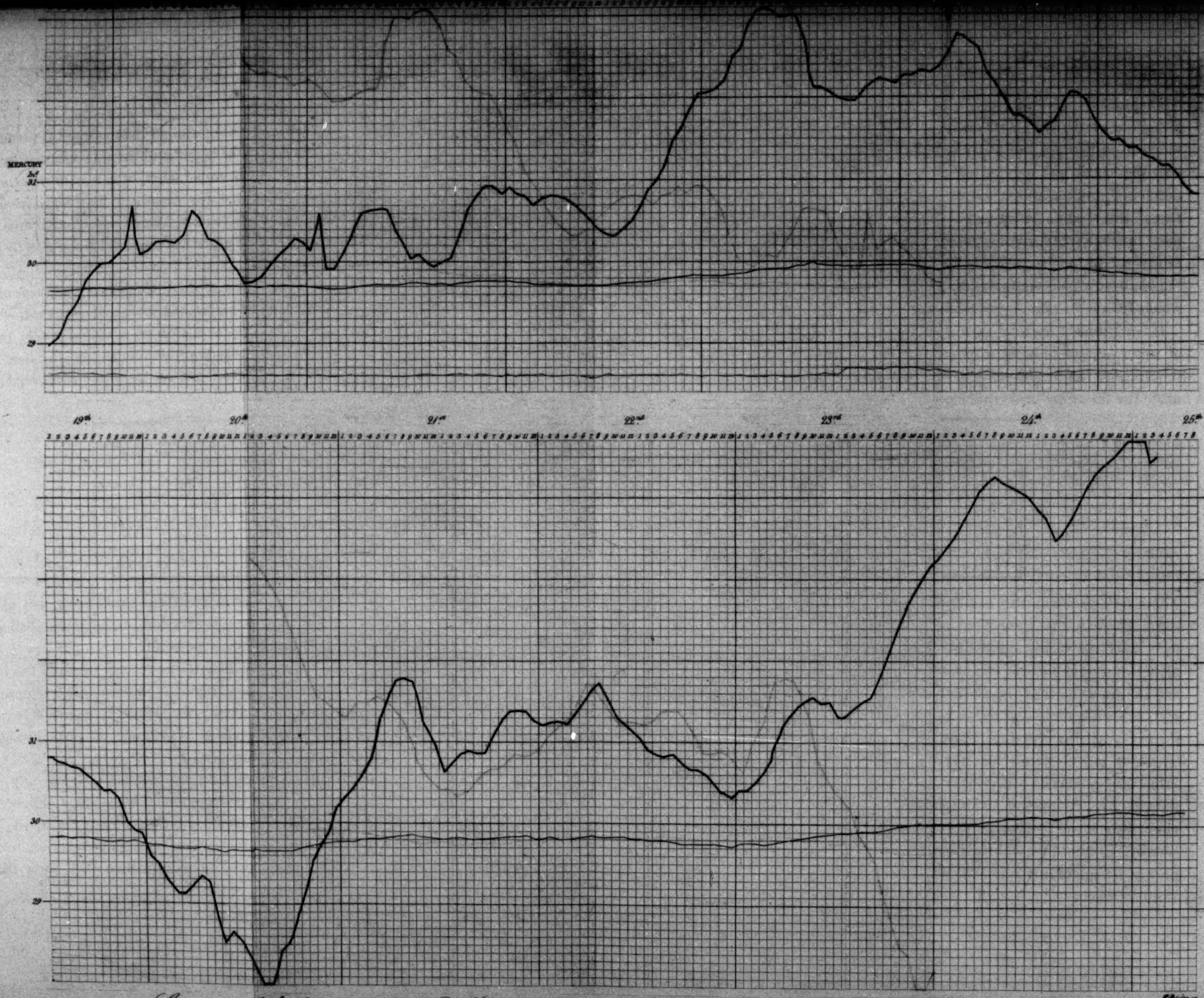
Upon removing the cover, we found that a portion of the liquid had by some means escaped, as, although the column of water stood considerably below the neutral point, the ivory point was not in contact with it. We carefully measured its distance, and found it to be 0·3 inch, to which, as the barometer stood at 385·94 inches, must be added 0·05 inch for the difference from the neutral point; and the amount 0·35 inch will be the quantity of the fluid deficient.

Upon examining the oil upon the surface, we found that it had undergone a very remarkable change. It was nearly covered with large clots of a mucilaginous-looking substance, which, in places, reached quite through to the water beneath; so that upon moving them aside the latter was uncovered. Upon the top of this, in various parts, were drops of an aqueous fluid, of a tenacious consistence, which had a very decided sweet taste, and resembled the substance which is formed during the process of saponification, to which the name of Glycerine has been given. There was also some carbonaceous matter, but not more than might probably be accounted for from depositions from the atmosphere. All these matters, with a great portion of the remaining oil, were carefully skimmed off, and the water beneath was found perfectly bright and transparent; there were no signs of metallic corrosion in any part, and every portion of the boiler, with its cover and brass-work, was as bright as on the day when they were put together.

We next examined the portion of oil and water which had been set by in a glass vessel for the purpose of watching any changes which it might undergo. This we found in a very different state. The stratum of oil upon the surface was rather more than an inch thick, and in this it differed from that in the boiler, which was not more than half an inch. The great body of it was perfectly bright and pure, and did not seem, from its taste, to have undergone any change, or to have acquired any rancidity. At the point of contact with the water it appeared to have undergone change, and to be separated from it by a tough film of the same mucilaginous-looking substance which we had found in the boiler. Upon agitating the glass, this film could be bent upwards without breaking; and a kind of fold was made in it of so tenacious a quality as to be some time before it again accommodated itself to the level of the liquid. Upon examination with a lens, it appeared to contain minute air-bubbles. These air-bubbles may have originated from some decomposition of the oil or water; but they were by no means numerous, and it is not at all improbable that they were the remains of a thin stratum of air included between the oil and the water; as there would be no perfect contact between the two liquids near the surface of the water. We next placed the glass, with its contents, under the receiver of an air-pump, and upon exhaustion of the air these little bubbles expanded and seemed to lift the film in parts and to escape with some difficulty through the oil. No air-bubbles, however, were formed in the mass of the subjacent water; proving that the water had been, in this instance, protected by the oil. Upon pushing the exhaustion to the utmost, a few insignificant bubbles were indeed extricated from a small flock of dust which had fallen to the bottom of the glass.

A little of the water was then taken out of the boiler in a glass vessel, which still retained a thin stratum of oil upon its surface. Upon exposing this to the action of the pump, air-bubbles in abundance were extricated from the whole mass, and it swelled up so as nearly to overflow the vessel in which it was contained; presenting a very marked contrast to the result of the previous experiment, and proving that the water in the boiler must have been strongly impregnated with gaseous matter. This examination took place on the 13th June, almost exactly two years from the completion of the water-barometer.

Upon consideration of all the circumstances, we were of opinion that the



J. Basire sc.

Comparison of the Oscillations of the Water Barometer with those of the Mercurial from hourly observations of Mr. Hudson in July 1831.

The dotted line represents the variations in the elasticity of the vapour. [illegible] formation of the mucilaginous-looking matter had opened a permeable communication between the water in the boiler and the atmosphere; by which not only the water was carried off by evaporation, which would account for the deficiency, but the air passed in and was absorbed: and we have little doubt that if the stratum of oil had been thicker, the change would have been confined to the lower surface, and the water would have been perfectly protected, as was the portion set aside in the glass.

I shall now proceed to notice two or three more circumstances of interest, which I remarked during my observation of the water-barometer.

It is extremely curious to watch its action in windy weather; the column of water appears to be in a perpetual motion, resembling the slow action of respiration. During a heavy gale of wind on the 16th of November 1830, I made the following observations:

Time.	Thermometers.		Water-Barometer.	Mercurial Barometer.
	Intern.	Extern.		
h m	°	°	Inches.	Inches.
2 30	56	55·5	387·87	29·092
2 45	....	....	387·59	29·090
3 0	....	....	387·44	29·090
3 15	....	....	387·28	29·090
4 0	....	....	387·64	29·090
4 15	....	....	387·85	29·090

About half-past two, the maximum range of the oscillations was about 0·28 inch; about half an hour later, one gust of wind caused an oscillation of 0·43 inch, and the minor oscillations were generally nearer the lower than the higher extreme. At four o'clock the movement became sensibly less in extent, and the mean point of the oscillations began to rise, and, as I ventured to predict, the wind very soon began to abate. It became very suddenly calm, and the next day was very fine. The time of this change, as indicated by the instrument, was certain within five minutes.

On the subjoined scale (Plate XX.) I have laid down the hourly observations of Mr. Hudson of the water and mercurial barometers obligingly communicated to me by that gentleman. They have not been corrected; but the corrections would be of little importance in the rough comparison which I at present design to institute. A very slight examination will show that there are many considerable oscillations of the aqueous column which are totally lost in the mercurial, and will prove that much curious information with regard to atmospheric changes might be derived from a long-continued series of such observations.

The most important result, however, and that which alone would have amply repaid all the labour expended upon the subject, is the fact pointed out by the observations of Mr. Hudson, that the water-barometer precedes by one hour the barometer of half-inch bore, and the latter the mountain barometer of 0·15-inch bore by the same interval, in their indications of the horary oscillations; showing that while philosophers are disputing about the hours of the maxima and minima, much depends upon the construction of the instruments observed; and proving the necessity, which I long ago pointed out, of making these delicate observations with instruments which have been compared with accurate and known standards. This comparative sluggishness of the mercurial barometer, when compared with the water, also proves that the difference between the two, when reduced by calculation of their specific gravities to the same expression, can only at times approximatively determine the elasticity of the included vapour; and that such determination must always be liable to a small error from this circumstance.

Should the Council of the Society hereafter come to the conclusion that there is enough of interest in the subject to induce them to prosecute it further, I am of opinion that the water-barometer might be reboiled and resealed without much risk; and I think that if a stratum of oil of four or five inches depth were afterwards poured upon the surface of the water, there would be little risk of the air again insinuating itself within it.

XXIV. *Hourly Observations on the Barometer; with experimental investigations into the phenomena of its periodical oscillation.* *By* James Hudson, *Assistant Secretary and Librarian to the Royal Society.* *Communicated by* John William Lubbock, *Esq. M.A. Vice President and Treasurer.*

Read June 21, 1832.

When Mr. Lubbock undertook, last year, an examination of the Meteorological Observations made daily at the Royal Society, during the preceding four years, he found that no satisfactory result connected with the diurnal variation of the barometer could be obtained from them, in consequence of the stated hours of observation not recurring after sufficiently small intervals of time. From the interesting nature of the phenomena of the barometer, and from the circumstance of no observations for determining the amount and peculiarities of its horary oscillation having been made at the Royal Society, I proposed to undertake as extensive a series of hourly observations on this instrument as my official duties and the state of my health would permit;—to prosecute such experimental investigations into collateral branches of the inquiry, as the anomalies presenting themselves might require;—and to institute, finally, a comparison between my own results and those derived from the labours of other observers, both in this country and on the Continent.

In endeavouring to accomplish these objects, I have been anxious in the first instance to present to the Society a series of observations, made at equal intervals of time,—in sufficient number,—through an extended period,—and with instruments, whose peculiarities of excellence or defect are well known and understood; and which, being conducted with every care, may furnish preliminary data for explaining the anomalies of its hourly and daily oscillation; determining, if possible, the laws which regulate its periodical changes; and ascertaining the circumstances which accelerate or retard the operation of these laws: being guided, in the progress of the inquiries, by the strict inductive intimations only of the results themselves, and without reference to any particular theory or current hypothesis.

I have now the honour of laying before the Society the first portion of these hourly observations, amounting to about three thousand in number, and made in the months of April, May, June and July of 1831, and in those of January and February of 1832. The Standard Barometer of the Society has been observed for about sixteen or eighteen hours during the day, through a period of seventy-five days; and also at every hour through the whole twenty-four hours for thirty days; the Water Barometer every hour, day and night, for fifteen days; and the Mountain Barometer also every hour, day and night, for the same period. In making these observations, no pains have been spared to ensure their accuracy; and I was enabled to extend the series through the whole twenty-four hours, with three barometers for fifteen days, and afterwards with one barometer for the same period, through the assistance of Mrs. HUDSON, who supplied my place as the observer for six hours of the night during these thirty days, and whose estimation in registering the instruments was found, on every comparison, to accord exactly with my own.

The Standard Barometer is fixed in the upper library, the Water Barometer within the public staircase, and the Mountain Barometer in the entrance-hall, of the Royal Society. Mr. BEVAN, of Leighton Bussard, was, in 1827, requested by a Committee of the Royal Society, of which he was also appointed a member, to determine the levels of the barometers then in the possession of the Society, above a fixed mark on Waterloo-bridge. From Mr. BEVAN'S report on that occasion, and from the additional information with which he had subsequently the kindness to furnish me on my application to him, I am enabled to lay before the Society the relative altitudes of the three barometers employed in my observations.

Mr. BEVAN adopted, as his bench-mark, the base of the columns of Waterloo-bridge, which base line, at that time, agreed nearly with the highest tide-line observed in the river, and was eleven feet six inches above the estimated mean level of the surface of the Thames at Greenwich. The presumed mean level above the sea at Sheerness was at the same time determined, from theoretical considerations, by the late Dr. YOUNG; and with an accuracy which, I am informed, has been confirmed in a remarkable manner by actual measurement.

The following Table exhibits the relative levels of the surfaces of the fluids in the cisterns of the barometers.

	Above the bench-mark on Waterloo-bridge.		Above the mean level of the Thames at Greenwich.		Above the mean level of the Sea (presumed).	
	ft.	in.	ft.	in.	ft.	in.
Standard Barometer ..	83	2½	94	9	95	0
Water Barometer	42	11	54	5½	54	8½
Mountain Barometer..	41	2½	52	9	53	0

The Standard Barometer was made by Newman, and placed in its present situation on December 12, 1822; and, at the request of a Committee of the Royal Society, it was constructed with great care under the direction of Mr. Daniell, who has, in his Meteorological Essays, given a full account of the mode and principles of its construction*. Its peculiar advantages are, a tube of great diameter, a cistern of unusual extent of surface, and an apparatus for determining the height of the mercurial column, so delicate and perfect, that, with the unassisted eye, it may be determined, on successive trials, with a difference only in the ten-thousandths of an inch. The cistern is a cylinder of turned mahogany, with an internal diameter of 5·3 inches, and which terminates above, in a rectangular pillar of polished mahogany, encasing the tube, 1¼ inch wide, and 2½ inches deep, rising 25½ inches above the level of the mercury, and bearing on its upper surface, and firmly screwed into it, a metallic plate, on which rests the brass scale, with the divisions and vernier. The tube has an internal diameter of 0·53 inch, and the neutral point of the instrument is 30·576 inches, at 54°.

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* I have been informed by Sir John Herschel, that the Royal Society's barometer has been compared, intermediately, with almost every other standard barometer in Europe. A fine mountain barometer, belonging to him, and made by Mr. Troughton, having being compared with it, previously to his setting out on an extensive tour on the Continent, in which it accompanied him, was found to give on his return, as Mr. Henderson related to me, exactly the same difference as that obtained before his leaving England, having been in the mean time the medium of comparison with a considerable number of Continental instruments. At his suggestion, I have opened a permanent registry for these standard comparisons. This barometer, with which Sir John Herschel had done me the honour of making some corresponding observations at Slough, is now entrusted to the care of Mr. Henderson, the Astronomer Royal at the Cape of Good Hope, who has promised to undertake with me a series of observations to be made simultaneously in that Colony and in London. Mr. Dunlop, the Astronomer Royal at Paramatta, in New South Wales, and Mr. Forbes, now on a scientific tour in Italy and Greece, will each, I have reason to believe, be able to undertake with me similar correspondent observations.

The Water Barometer forms the subject of a paper by Mr. DANIELL, printed in the present volume of the Transactions, and containing a full statement of its peculiarities and the mode of its construction.

The Mountain Barometer is the property of Mr. DANIELL, and is considered by him as an almost perfect instrument. It has a tube of 0·15 inch and a cistern of 1·2 inch internal diameter, with a brass scale extending to the surface of the mercury in the cistern; and is the first barometer to which Mr. DANIELL applied the platina guard for preventing the insinuation of air into the vacuum chamber of the instrument. Its neutral point is 30·080, at 65°.

The regularity with which the barometer, in tropical climates, proceeds in its periodical rise and fall from day to day with almost uninterrupted progression, has long been observed by our travellers and philosophers. This periodical oscillation, as the parallel of observation becomes more remote from the equator, gradually ceases to be obvious in the observations of a single day; and in its place we have the violent and irregular movements of the mercurial column, so well known in our own and other extra-tropical climates, and in which the effect of no constant law is apparent. By classing, however, the observations made at the same hours on several successive days, and deriving from their union the hours of one mean day, it has been found that these accidental variations destroy or neutralize each other, and allow the constant, or equatorial, oscillation to become appreciable and subject to investigation *. The results now presented to the Society consist of eight such mean days, each of them derived from observations made on fifteen days, a period I have adopted as the standard, and which appears to be amply extensive for clearing the result from the interference of the accidental variations. In forming each mean day, all the observations made at a given hour, on successive days, have been collected together, their sum taken, and a mean result for the given hour obtained by dividing that sum by fifteen, the number of the observations. A mean hourly result for the temperature has been obtained in the same manner. Having thus derived a mean quantity for each

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* The clear and striking statement of these phenomena, given by Sir JOHN HERSCHEL in his Preliminary Discourse, (§ 228.) suggested the original idea of the present observations.

hour of the mean day, a total mean of the whole of the observations made during the given period, has then been obtained, and each mean hourly quantity being referred to it, the hourly variations from this general mean have been determined. These hourly results are detailed in eleven Tables.

In the first five sets of fifteen days' observations the instruments were registered as nearly at the exact hour as was found to be practicable, and as few of the observations were omitted to be taken as circumstances would allow. The mean times of observation are therefore given in these five sets; and where an hour has passed unobserved, the place of a real observation has been supplied by a mean quantity derived from the two nearest observations. I have reason to believe, from a variety of trials which I have made, that when the interval of time elapsed is short, and the omission of an observation occurs only occasionally, and without periodical recurrence, that this mode of supplying the vacancy, not by an arbitrary quantity but a derived mean, is by far the simplest and best, and less injurious to the result than that of allowing the vacancy to remain unoccupied *. In the Tables the amount of such interpolations is stated; and from the number of the observations, and the small extent of possible error which could be made, it is probable that the mean result is little, if at all, different from that which an entirely unbroken series of observations during these five periods would have given. In the remaining three sets, the observations were made in every instance at the complete hour, and without the omission of a single observation.—The corrections have been applied to the mean results of the observations. Those of the Standard Barometer have been corrected for the relative superficial capacities of the cistern and the tube, for the constant amount of capillary depression (—·004), and for temperature. The Mountain Barometer, in addition to these, (the capillary depression being assumed as = —·044) has been corrected for its brass continuous scale. The Reduction Tables for the English Barometer, drawn up and published under the direction of Professor SCHUMACHER, first in his Sammlung von Hülfstafeln, and afterwards, with the brass scale referred, at Mr. BAILY's suggestion, to

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* In the former case, the error is limited by the small extent of the *hourly* oscillation; in the latter, it extends to the mean *daily* variation at the particular hour for the given period. This daily mean, so widely remote in general from the single hourly observation, is, in effect, by this last process, made the substitute of it,—the mean of any set of quantities being equal to the mean of such quantities increased in number by the addition of the former mean.

the standard temperature of 62°, in the fifth volume of the Astronomische Nachrichten, have been employed to reduce the results of the observations to zero *. The first of these tables is intended for those instruments which are not supplied with a brass scale, and has reference only to the expansion of mercury. By this table the observations of the Standard Barometer have been reduced to 32° F. The second table is intended for those instruments which are furnished with a continuous brass scale, the temperature of which it reduces to 62° F., (the standard temperature of the English linear measures,) and the mercury to 32°, as before. The observations of the Mountain Barometer have been reduced by this second table. The observations with the Water Barometer have been corrected only for the expansive power of the vapour in its vacuum chamber at the temperature of the thermometer attached to the vernier, by Mr. Dalton's Table, given in Dr. Henry's Elements of Chemistry, and adapted to the present purpose by assuming the mean specific gravity of mercury (that of the Standard Barometer) as 13·624.

First set of fifteen days' Observations. April 26th to May 10th, 1831.

Mean Times of Observation.	Number of Observations at each hour.	Number of Interpolations.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from Mean.	Difference of Thermometer from Mean.
h m			inches.	°	inches.		
A.M. 9 0	15	0	29·720	56·7	29·641	+·007	−0·5
10 4	10	5	29·718	57·6	29·636	+·002	+0·4
11 4	14	1	29·713	58·2	29·630	−·004	+1·0
12 3	9	6	29·710	58·6	29·625	−·009	+1·4
P.M. 1 6	12	3	29·708	58·8	29·623	−·011	+1·6
2 7	12	3	29·704	59·0	29·618	−·016	+1·8
3 0	15	0	29·694	58·9	29·608	−·026	+1·7
4 4	10	5	29·694	58·7	29·609	−·025	+1·5
5 2	11	4	29·696	58·2	29·613	−·021	+1·0
6 3	9	6	29·701	57·6	29·619	−·015	+0·4
7 2	7	8	29·708	56·9	29·628	−·006	−0·3
8 2	9	6	29·718	56·2	29·641	+·007	−1·0
9 2	9	6	29·725	55·7	29·649	+·015	−1·5
10 2	8	7	29·729	55·3	29·655	+·021	−1·9
11 2	8	7	29·733	54·6	29·661	+·027	−2·6
12 3	9	6	29·737	54·2	29·667	+·033	−3·0
Mean....	10	5	29·713	57·2	29·634		

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* I am indebted to the liberality and kindness of Professor Schumacher for fifty copies of these valuable Tables, for distribution among such meteorological observers in this country as may feel desirous of possessing them.

Second set of fifteen days' Observations. May 11th to May 25th, 1831.

Mean Times of Observation.	Number of Observations at each hour.	Number of Interpola-tions.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from Mean.	Difference of Thermometer from Mean.
h m			inches.	°	inches.		
A.M. 9 0	15	0	30·004	62·1	29·912	+·025	−0·2
10 5	15	0	30·000	62·5	29·907	+·020	+0·2
11 3	14	1	29·997	62·6	29·903	+·016	+0·3
12 3	12	3	29·994	62·9	29·900	+·013	+0·6
P.M. 1 3	12	3	29·989	63·2	29·894	+·007	+0·9
2 4	13	2	29·983	63·5	29·887	·000	+1·2
3 1	15	0	29·972	63·9	29·875	−·012	+1·6
4 1	12	3	29·970	63·9	29·873	−·014	+1·6
5 2	14	1	29·965	63·7	29·872	−·015	+1·4
6 2	10	5	29·965	63·2	29·870	−·017	+0·9
7 6	11	4	29·969	62·6	29·875	−·012	+0·3
8 6	9	6	29·972	61·9	29·881	−·006	−0·4
9 7	9	6	29·978	61·3	29·888	+·001	−1·0
10 3	8	7	29·980	60·8	29·892	+·005	−1·5
11 4	9	6	29·978	60·2	29·892	+·005	−2·1
12 1	8	7	29·963	59·6	29·878	−·009	−2·7
Mean	12	3	29·980	62·3	29·887		

Third set of fifteen days' Observations. May 26th to June 9th, 1831.

Mean Times of Observation.	Number of Observations at each hour.	Number of Interpola-tions.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from Mean.	Difference of Thermometer from Mean.
h m			inches.	°	inches.		
A.M. 9 0	15	0	30·010	63·4	29·914	+·024	−0·8
10 2	14	1	30·010	63·7	29·913	+·023	−0·5
11 1	15	0	30·010	64·1	29·912	+·022	−0·1
12 2	13	2	30·005	64·3	29·906	+·016	+0·1
P.M. 1 3	14	1	29·993	65·2	29·892	+·002	+1·0
2 0	15	0	29·988	65·7	29·885	−·005	+1·5
3 0	15	0	29·982	66·0	29·878	−·012	+1·8
4 3	14	1	29·977	66·0	29·873	−·017	+1·8
5 1	14	1	29·973	65·6	29·870	−·020	+1·4
6 4	15	0	29·973	65·2	29·872	−·018	+1·0
7 0	14	1	29·971	64·6	29·871	−·019	+0·4
8 3	13	2	29·977	63·7	29·880	−·010	−0·5
9 3	14	1	29·985	63·0	29·890	·000	−1·2
10 2	12	3	29·985	62·6	29·891	+·001	−1·6
11 1	14	1	29·987	62·2	29·895	+·005	−2·0
11 49	14	1	29·982	62·0	29·890	·000	−2·2
Mean	14	1	29·988	64·2	29·890		

Fourth set of fifteen days' Observations. June 10th to June 24th, 1831.

Mean Times of Observation.	Number of Observations at each hour.	Number of Interpolations.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from Mean.	Difference of Thermometer from Mean.
h m			inches.	°	inches.		
A.M. 6 4	6	9	29·996	69·3	29·882	—·002	+0·3
7 2	8	7	29·997	70·4	29·880	—·004	+1·4
8 4	10	5	29·994	70·2	29·878	—·006	+1·2
9 0	15	0	29·992	69·8	29·877	—·007	+0·8
10 1	14	1	29·992	69·5	29·878	—·006	+0·5
11 2	14	1	29·992	69·0	29·879	—·005	0·0
12 1	14	1	29·992	68·9	29·880	—·004	—0·1
P.M. 1 0	14	1	29·992	69·1	29·880	—·004	+0·1
2 0	14	1	29·996	68·7	29·884	·000	—0·3
3 0	15	0	29·997	69·5	29·883	—·001	+0·5
4 2	13	2	29·997	69·6	29·882	—·002	+0·6
5 3	14	1	29·994	69·5	29·880	—·004	+0·5
6 0	14	1	29·995	69·2	29·882	—·002	+0·2
7 2	14	1	29·997	68·7	29·885	+·001	—0·3
8 3	12	3	30·003	68·1	29·893	+·009	—0·9
9 5	14	1	30·008	67·5	29·900	+·016	—1·5
10 0	13	2	30·007	67·2	29·900	+·016	—1·8
11 2	12	3	30·003	66·7	29·897	+·013	—2·3
Mean....	12	3	29·997	69·0	29·884		

Fifth set of fifteen days' Observations. June 24th to July 13th, 1831.

Mean Times of Observation.	Number of Observations at each hour.	Number of Interpolations.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from mean.	Difference of Thermometer from mean.
h m			inches.	°	inches.		
A.M. 6 12	11	4	29·995	64·6	29·896	+·020	—2·9
7 2	15	0	30·000	65·8	29·897	+·021	—1·7
8 6	15	0	30·000	66·2	29·896	+·020	—1·3
9 0	15	0	29·998	66·7	29·892	+·016	—0·8
10 1	15	0	29·998	67·2	29·891	+·015	—0·3
11 2	15	0	29·993	67·7	29·884	+·008	+0·2
12 5	13	2	29·988	68·0	29·878	+·002	+0·5
P.M. 1 0	14	1	29·984	68·4	29·873	—·003	+0·9
2 3	15	0	29·979	68·7	29·867	—·009	+1·2
3 0	15	0	29·975	69·0	29·862	—·014	+1·5
4 5	15	0	29·970	69·2	29·857	—·019	+1·7
5 2	15	0	29·968	69·1	29·855	—·021	+1·6
6 2	14	1	29·968	68·9	29·856	—·020	+1·4
7 2	14	1	29·970	68·1	29·861	—·015	+0·6
8 3	15	0	29·974	67·9	29·864	—·012	+0·4
9 3	14	1	29·982	67·3	29·874	—·002	—0·2
10 7	10	5	29·986	66·8	29·880	+·004	—0·7
11 4	10	5	29·989	66·2	29·885	+·009	—1·3
Mean....	14	1	29·984	67·5	29·876		

Sixth set of fifteen days' Observations. July 14th to July 28th, 1831.

I. Water Barometer.

Times of Observation.		Number of Observations at each hour.	Water Barometer.	Attached Thermometer.	Immersed Thermometer.	Subjacent Thermometer.	Water Barometer corrected for vapour.	Difference of Water Barometer from Mean.
	h		inches.	°	°	°	inches.	
A.M.	1	15	392·979	64·3	64·4	63·9	401·194	—·114
	2	15	392·985	64·1	64·3	63·9	401·146	—·162
	3	15	393·019	63·9	64·3	63·9	401·125	—·183
	4	15	393·055	63·9	63·9	63·8	401·161	—·147
	5	15	393·198	63·6	63·6	63·8	401·223	—·085
	6	15	393·348	63·3	63·3	63·8	401·304	—·004
	7	15	393·464	63·1	63·0	63·8	401·366	+·058
	8	15	393·469	63·0	62·9	63·8	401·344	+·036
	9	15	393·384	63·4	62·9	63·8	401·368	+·060
	10	15	393·255	63·8	62·8	63·8	401·334	+·026
	11	15	393·081	64·4	63·0	63·8	401·324	+·016
	12	15	392·901	65·2	63·4	63·9	401·348	+·040
P.M.	1	15	392·640	66·1	64·0	64·0	401·318	+·010
	2	15	392·494	66·7	64·4	64·0	401·336	+·028
	3	15	392·430	66·5	64·8	64·1	401·217	—·091
	4	15	392·391	66·5	65·1	64·2	401·178	—·130
	5	15	392·386	66·5	65·3	64·2	401·173	—·135
	6	15	392·457	66·4	65·4	64·3	401·217	—·091
	7	15	392·591	66·2	65·4	64·3	401·297	—·011
	8	15	392·804	65·8	65·3	64·3	401·401	+·093
	9	15	392·969	65·4	65·3	64·3	401·470	+·162
	10	15	393·046	65·3	65·2	64·2	401·520	+·212
	11	15	393·116	65·0	65·1	64·2	401·508	+·200
	12	15	393·157	64·9	65·0	64·2	401·522	+·214
Mean....			392·943	64·9	64·3	64·0	401·308	

The attached thermometer is let into the moveable brass cylinder connected with the vernier and encasing the outside of the glass tube.

The immersed thermometer is secured within the tube of the barometer, a few feet below the general surface of the column of the water.

The subjacent thermometer, by NEWMAN, was placed immediately under the cistern of the barometer, and, its variations being found so very inconsiderable, it was registered only at intervals of four or five hours during the day, and the series completed for each hour by interpolation.

Sixth set of fifteen days' Observations. July 14th to July 28th, 1831.

II. Royal Society's Standard Barometer.

Times of Observation.	Number of Observations at each hour.	Barometer.	Attached Thermometer.	Thermometer at Vacuum Chamber.	Barometer reduced to 32°.	Difference of Barometer from Mean.
h		inches.	°	°	inches.	
A.M. 1	15	29·917	65·9	65·0	29·812	—·004
2	15	29·913	65·6	64·7	29·809	—·007
3	15	29·911	65·4	64·5	29·808	—·008
4	15	29·902	65·5	64·4	29·799	—·017
5	15	29·907	65·3	64·5	29·804	—·012
6	15	29·915	65·6	65·3	29·811	—·005
7	15	29·924	66·3	66·3	29·818	+·002
8	15	29·928	67·0	66·7	29·821	+·005
9	15	29·931	68·3	67·7	29·820	+·004
10	15	29·932	68·6	68·0	29·820	+·004
11	15	29·931	69·6	68·2	29·816	·000
12	15	29·931	69·9	68·3	29·816	·000
P.M. 1	15	29·929	70·0	68·7	29·813	—·003
2	15	29·927	70·2	69·0	29·811	—·005
3	15	29·924	70·4	69·0	29·806	—·010
4	15	29·922	70·4	68·8	29·804	—·012
5	15	29·918	70·2	68·5	29·801	—·015
6	15	29·919	70·0	68·0	29·802	—·014
7	15	29·923	69·5	67·5	29·808	—·008
8	15	29·932	68·7	66·7	29·820	+·004
9	15	29·944	67·9	66·5	29·834	+·018
10	15	29·946	67·4	66·0	29·838	+·022
11	15	29·948	66·9	65·8	29·842	+·026
12	15	29·947	66·6	65·6	29·841	+·025
		29·926	68·0	66·8	29·816	

The thermometer placed in contact with that portion of the glass tube of the Standard Barometer forming its vacuum chamber, was a very delicate instrument, made by Crichton.

Sixth set of fifteen days' Observations. July 14th to July 28th, 1831.

III. Mountain Barometer.

Times of Observation.	Number of Observations at each hour.	Barometer.	Attached Thermometer.	Barometer reduced.	Difference of Barometer from Mean.
h		inches.	°	inches.	
A.M. 1	15	29·871	63·9	29·816	—·007
2	15	29·867	63.7	29·813	—·010
3	15	29·865	63·3	29·813	—·010
4	15	29·861	63·0	29·809	—·014
5	15	29·862	62·7	29·810	—·013
6	15	29·868	62·7	29·816	—·007
7	15	29·874	62·8	29·822	—·001
8	15	29·877	63·1	29·824	+·001
9	15	29·878	63·7	29·824	+·001
10	15	29·879	64·0	29·824	+·001
11	15	29·878	64·2	29·822	—·001
12	15	29·882	65·1	29·824	+·001
P.M. 1	15	29·883	65·4	29·824	+·001
2	15	29·879	65·7	29·819	—·004
3	15	29·877	65·8	29·817	—·006
4	15	29·874	65·9	29·814	—·009
5	15	29·872	65·6	29·813	—·010
6	15	29·873	66·0	29·813	—·010
7	15	29·878	65·8	29·818	—·005
8	15	29·887	65·6	29·828	+·005
9	15	29·896	65·4	29·838	+·015
10	15	29·899	65·1	29·842	+·019
11	15	29·902	64·9	29·845	+·022
12	15	29·901	64·6	29·845	+·022
Mean....		29·879	64·5	29·823	

The direction of the wind and state of the sky were also registered every hour daily, from 3 A.M. to 9 P.M., and striking changes in the weather noted, during these fifteen days.

Corresponding Variations of the Water, Standard, and Mountain Barometers; and their Thermometers.

Times of Observation.	Water Barometer reduced to the standard of Mercury.	Attached Thermometer.	Immersed Thermometer.	Subjacent Thermometer.	Royal Society's Standard Barometer.	Attached Thermometer.	Thermometer at Vacuum Chamber.	Mountain Barometer.	Attached Thermometer.
h		°	°	°		°	°		°
A.M. 1	−·008	−0·6	+0·1	−0·1	−·004	−2·1	−1·8	−·007	−0·6
2	−·012	−0·8	0·0	−0·1	−·007	−2·4	−2·1	−·010	−0·8
3	−·013	−1·0	0·0	−0·1	−·008	−2·6	−2·3	−·010	−1·2
4	−·011	−1·0	−0·4	−0·2	−·017	−2·5	−2·4	−·014	−1·5
5	−·006	−1·3	−0·7	−0·2	−·012	−2·7	−2·3	−·013	−1·8
6	·000	−1·6	−1·0	−0·2	−·005	−2·4	−1·5	−·007	−1·8
7	+·004	−1·8	−1·3	−0·2	+·002	−1·7	−0·5	−·001	−1·7
8	+·003	−1·9	−1·4	−0·2	+·005	−1·0	−0·1	+·001	−1·4
9	+·004	−1·5	−1·4	−0·2	+·004	+0·3	+0·9	+·001	−0·8
10	+·002	−1·1	−1·5	−0·2	+·004	+0·6	+1·2	+·001	−0·5
11	+·001	−0·5	−1·3	−0·2	·000	+1·6	+1·4	−·001	−0·3
12	+·003	+0·3	−0·9	−0·1	·000	+1·9	+1·5	+·001	+0·6
P.M. 1	+·001	+1·2	−0·3	0·0	−·003	+2·0	+1·9	+·001	+0·9
2	+·002	+1·8	+0·1	0·0	−·005	+2·2	+2·2	−·004	+1·2
3	−·007	+1·6	+0·5	+0·1	−·010	+2·4	+2·2	−·006	+1·3
4	−·009	+1·6	+0·8	+0·2	−·012	+2·4	+2·0	−·009	+1·4
5	−·010	+1·6	+1·0	+0·2	−·015	+2·2	+1·7	−·010	+1·1
6	−·007	+1·5	+1·1	+0·3	−·014	+2·0	+1·2	−·010	+1·5
7	−·001	+1·3	+1·1	+0·3	−·008	+1·5	+0·7	−·005	+1·3
8	+·007	+0·9	+1·0	+0·3	+·004	+0·7	−0·1	+·005	+1·1
9	+·012	+0·5	+1·0	+0·3	+·018	−0·1	−0·3	+·015	+0·9
10	+·016	+0·4	+0·9	+0·2	+·022	−0·6	−0·8	+·019	+0·6
11	+·015	+0·1	+0·8	+0·2	+·026	−1·1	−1·0	+·022	+0·4
12	+·016	0·0	+0·7	+0·2	+·025	−1·4	−1·2	+·022	+0·1
Mean...	29·508	64·9	64·3	64·0	29·816	68·0	66·8	29·823	64·5

Seventh set of fifteen days' Observations. Jan. 17th to Jan. 31st, 1832[*].

Times of Observation.	Number of Observations at each hour.	Barometer.	Attached Thermometer.	Barometer reduced to 32°.	Difference of Barometer from mean.	Difference of Thermometer from mean.
h		inches.	°	inches.		
A.M. 1	15	30·190	44·0	30·154	+·018	0·0
2	15	30·185	44·1	30·149	+·013	+0·1
3	15	30·181	44·1	30·145	+·009	+0·1
4	15	30·178	44·0	30·142	+·006	0·0
5	15	30·173	44·0	30·137	+·001	0·0
6	15	30·172	43·9	30·136	·000	−0·1
7	15	30·173	43·8	30·138	+·002	−0·2
8	15	30·178	43·7	30·143	+·007	−0·3
9	15	30·185	43·6	30·150	+·014	−0·4
10	15	30·193	43·6	30·158	+·022	−0·4
11	15	30·194	43·6	30·159	+·023	−0·4
12	15	30·183	43·6	30·148	+·012	−0·4
P.M. 1	15	30·168	43·9	30·132	−·004	−0·1
2	15	30·160	44·1	30·124	−·012	+0·1
3	15	30·160	44·2	30·123	−·013	+0·2
4	15	30·159	44·2	30·122	−·014	+0·2
5	15	30·161	44·2	30·124	−·012	+0·2
6	15	30·163	44·3	30·126	−·010	+0·3
7	15	30·165	44·2	30·128	−·008	+0·2
8	15	30·167	44·1	30·131	−·005	+0·1
9	15	30·165	44·1	30·129	−·007	+0·1
10	15	30·163	44·2	30·126	−·010	+0·2
11	15	30·164	44·3	30·127	−·009	+0·3
12	15	30·156	44·2	30·119	−·017	+0·2
Mean....		30·172	44·0	30·136		

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[*] The Rev. Mr. HUSSEY made, during this period, fifty-five corresponding observations at the Rectory at Chiselhurst, with one of FORTIN's best barometers. I am indebted to his kindness for a copy of these observations, and, on a future occasion, I propose to compare them with my own.

Eighth set of fifteen days' Observations. Feb. 6th to Feb. 20th, 1832.

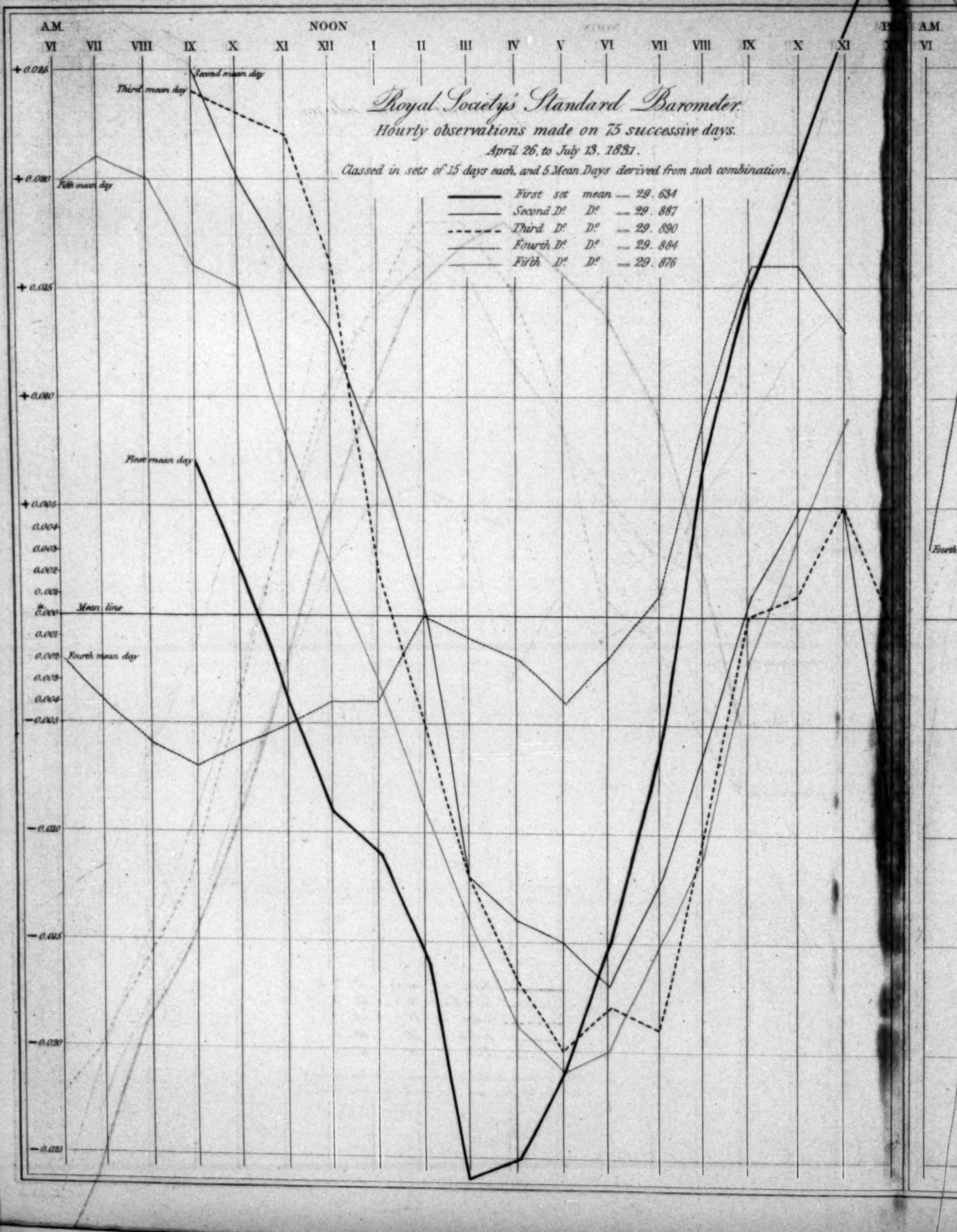
Times of Observation.	Number of Observations at each hour.	Barometer.	Attached Thermometer.	External Thermometer.	Barometer reduced to 32°.	Difference of Barometer from mean.	Difference of attached Thermometer from mean.	Difference of external Thermometer from mean.
h		inches.	°	°	inches.		°	°
A.M. 8	15	30·144	42·7	37·6	30·112	+·001	−1·4	−2·6
9	15	30·149	43·0	38·1	30·116	+·005	−1·1	−2·1
10	15	30·154	43·3	39·2	30·120	+·009	−0·8	−1·0
11	15	30·156	43·7	40·3	30·121	+·010	−0·4	+0·1
12	15	30·149	44·2	41·4	30·112	+·001	+0·1	+1·2
P.M. 1	15	30·141	44·6	42·1	30·103	−·008	+0·5	+1·9
2	15	30·132	45·0	41·8	30·093	−·018	+0·9	+1·6
3	15	30·131	45·1	42·4	30·092	−·019	+1·0	+2·2
4	15	30·132	45·0	41·8	30·093	−·018	+0·9	+1·6
5	15	30·138	44·8	41·3	30·100	−·011	+0·7	+1·1
6	15	30·147	44·4	40·8	30·110	−·001	+0·3	+0·6
7	15	30·152	44·2	40·3	30·115	+·004	+0·1	+0·1
8	15	30·153	43·9	39·9	30·117	+·006	−0·2	−0·3
9	15	30·154	43·8	39·6	30·119	+·008	−0·3	−0·6
10	15	30·155	43·8	39·2	30·120	+·009	−0·3	−1·0
11	15	30·155	43·7	38·8	30·120	+·009	−0·4	−1·4
12	15	30·156	43·6	38·4	30·121	+·010	−0·5	−1·8
Mean		30·147	44·1	40·2	30·111			

The external thermometer registered during this period was very accurate and sensible, and constructed many years ago by Nairne.

The most striking results which these observations have afforded, are exhibited, by means of linear representations, in the four Plates which accompany this paper. The respective variations from each general mean are referred, according to a given scale, to the mean line, and their points of distance from it, at each successive hour, are connected together by means of straight lines. The barometrical changes, and the variations of temperature, are each referred to the same scale, ·001 of an inch in the former case being equal to ·1 of a degree in the latter.

Plate XXI. represents the mean hourly variations of the Standard Barometer, and also those of the Attached Thermometer, in the first five sets of observations; and displays,—

1. The general similarity of character, and of amount, in the mean variations, compared with the irregular changes of the barometer under ordinary circumstances.

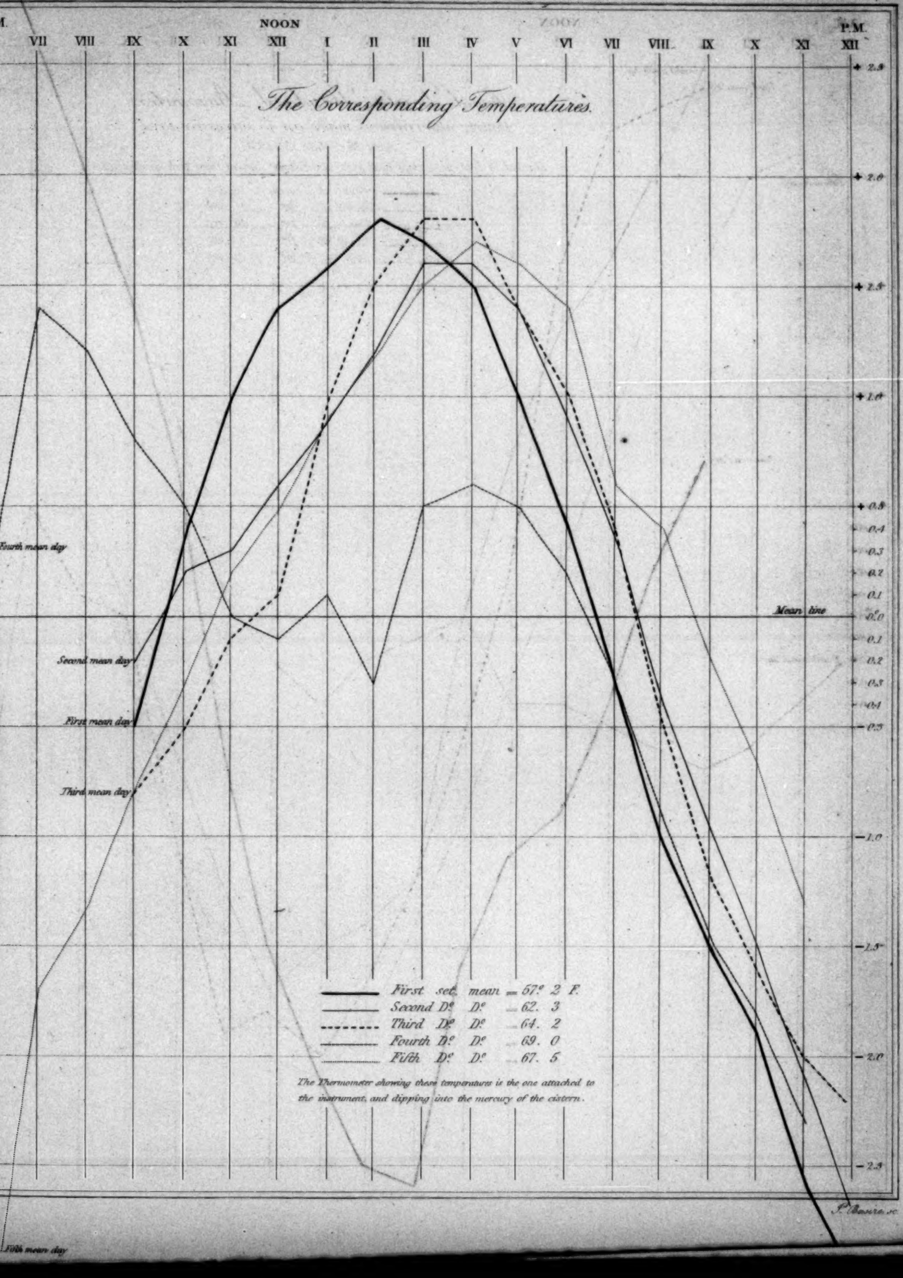


Royal Society's Standard Barometer.

Hourly observations made on 75 successive days.

April 26, to July 13, 1831.

Classed in sets of 15 days each, and 5 Mean Days derived from such combination.

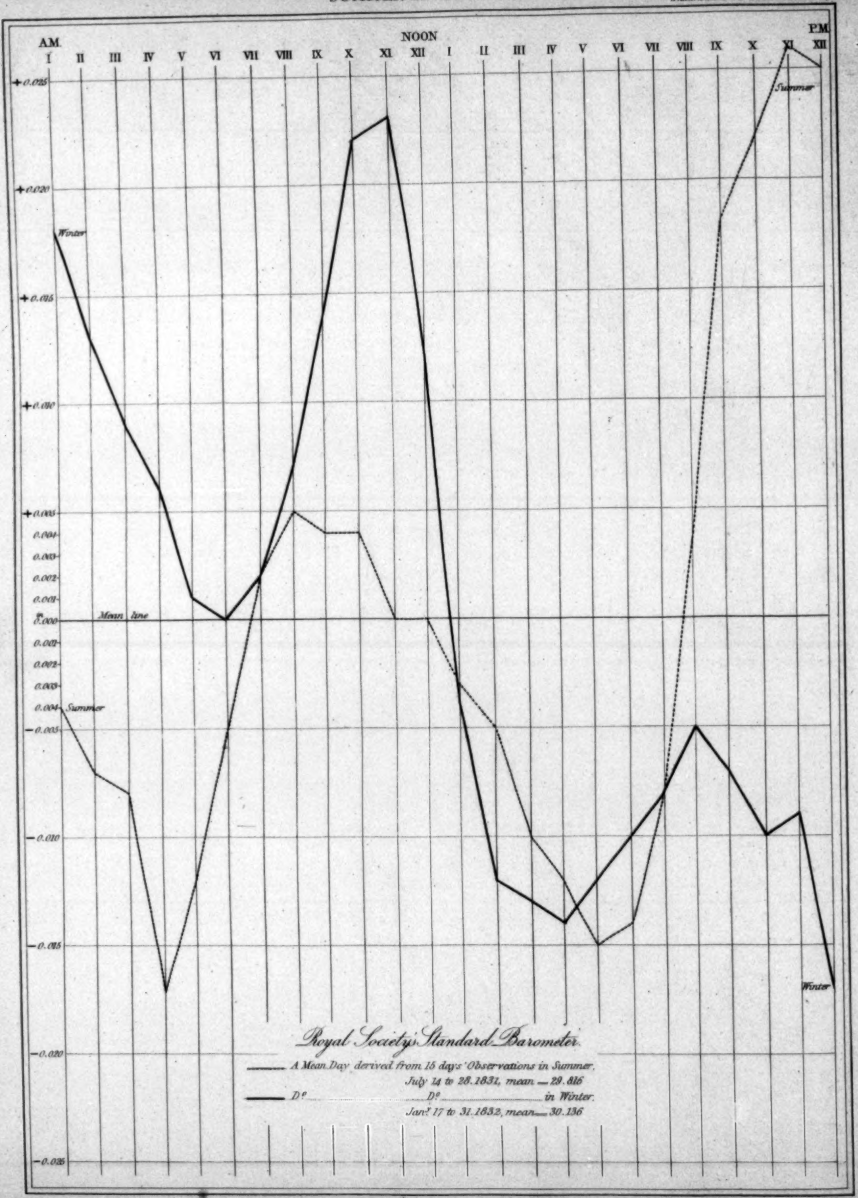


The Corresponding Temperatures.

The Thermometer showing these temperatures is the one attached to the instrument, and dipping into the mercury of the cistern.

J. Basire sc.

SUMMER and WINTER.



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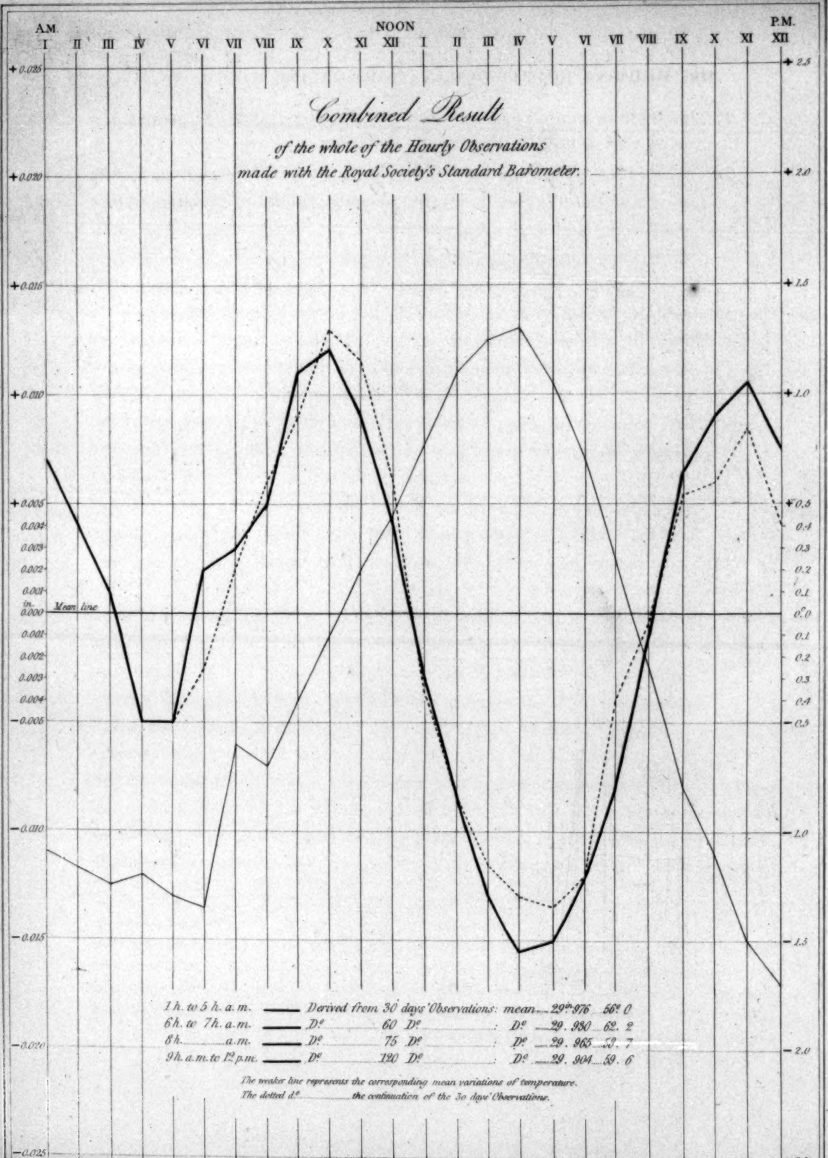

Combined Result

of the whole of the Hourly Observations
made with the Royal Society's Standard Barometer.

1 h. to 5 h. a. m. —— Derived from 30 days' Observations: mean — 29in. 976 56°. 0
6 h. to 7 h. a. m. —— D° .... 60 D° .... : D° — 29. 930 — 62. 2
8 h. .... a. m. —— D° .... 75 D° .... : D° — 29. 965 — 58. 7
9 h. a. m. to 12 p. m. —— D° .... 120 D° .... : D° — 29. 904 — 59. 6

The weaker line represents the corresponding mean variations of temperature.
The dotted d° .... the continuation of the 30 days' Observations.

J. Basire sc.

2. The striking connection between the barometrical changes and the variations of temperature.
3. The relation which appears to subsist between the variations before noon and those before midnight,—a great amount of variation before noon being followed, in the same mean day, by a corresponding small variation before midnight, and the contrary.

Plate XXII. exhibits the simultaneous movements of the Water Barometer, the Standard Barometer, and the Mountain Barometer; and points out,—

4. The general accordance in the mean variations of three instruments, so dissimilar in principle and construction; and the remarkable nature of those differences which their simultaneous observation has elicited.
5. The precession in time, by about an hour, of the mean motions of the Water Barometer over those of the Standard Barometer; and the precession, by the same interval, of the mean changes of this latter instrument, over those of the Mountain Barometer*.

Plate XXIII. exhibits a comparative view of a mean day's observations in summer, with one in winter, after an interval of exactly half a year; and displays,—

6. The influence which the season of the year, or the temperature of such season, appears to exercise over the hours of maximum and minimum, and over the amount of the mean variations. The minimum and maximum of the morning are earlier, and those of the evening later, in summer than in winter: and the variations in summer are small about noon, and great about midnight; those in winter, the reverse.

Plate XXIV. represents the mean result of the whole of the observations. The mean variations of the first five hours are referred to a general mean derived from all observations made continuously from 1 A.M. to midnight; those of the next two hours are referred to one derived from all observations made from 6 A.M. to midnight; the mean variation at 8 A.M. is referred to a general mean derived from all observations made from that hour till midnight; and the variations at each of the subsequent hours are referred to a mean of all observations made from 9 A.M. till midnight*. It points out,—

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* I am not aware that any series of observations has before exhibited this singular result, and developed the important influence which the diameter of the tube, and the nature of the fluid column exercise over the *changes* which the atmospheric pressure ought to produce in the barometer. Dr. Prout has since informed me, that he has found a barometer made with sulphuric acid move with much greater freedom than the ordinary mercurial barometers,—a fact which he considers only to be explained by the greater mobility of the molecules of the liquid under these circumstances, and which strikingly corroborates this result of my observations.

7. That the greatest of all the mean variations is nearly ·016 inch in amount, and occurs in the afternoon minimum height of the barometer at four o'clock; the next ·012 inch, and found in the forenoon maximum at ten o'clock; after this the one of nearly ·011 inch in the evening maximum at eleven o'clock; and finally that of ·005 inch occurring in the morning minimum at half-past four.
8. That the general relation between the barometrical changes and the variations of temperature, appears to be direct during the morning hours, and inverse during those of the day and evening.
9. The singular fact, that while a period of fifteen days gives a mean day generally distinguished by its relative variations at noon and midnight, a period of one month, or a complete lunation, not only gives a gradual succession of variations, but, in all these observations, a result almost identical in character and amount with the combined result of the whole.

Among the investigations in which I am at present engaged, are those relating to the following inquiries: 1. To ascertain whether the mercurial vapour in the vacuum chamber of the barometer, sensibly influences the height of the column at the ordinary variations of the temperature of the atmosphere. 2. Whether the Tables for the reduction of the temperature of the mercury to zero are practically accurate. 3. A full investigation into the influence which the diameter of the tube exercises over the fluid column. 4. The relation between the mean daily variation of the magnetic needle and that of the barometer; and whether the former would be found to exhibit the same dependence upon changes of temperature as the present observations have shown the latter to have. 5. The connexion between the mean barometric height and the amount of the variations referred to it, and the influence of altitude in the station of observation upon the variations. 6. A complete examination of the effect of temperature in influencing the changes of the barometer. 7. Whether, after the application of the ordinary corrections, the changes in the length of the mercurial column correspond accurately with those which take place in its absolute weight.

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* I find that a mean derived from all the observations of the twenty-four hours, compared with one derived from all those of the sixteen hours, from 9 A.M. to 12 P.M., of the same period of observation, differs from it only by ·001 of an inch.

With regard to the first of these inquiries, Mr. Dollond has, at his own expense, furnished me with an instrument exhibiting the changes of atmospheric pressure without involving the agency of the mercurial vapour; and with which I propose to make a series of observations, simultaneously with the Standard Barometer. It is a Baroscope of considerable dimensions, and the same in principle as the well-known instrument of Boyle, having a thin glass globe, of one foot and a quarter in diameter, counterpoised by a solid sphere of lead. From an abstract of a memoir by Signor Avogadro, contained in the fourth number of the Annales de Chimie for the present year, on the elastic force of mercurial vapour at different temperatures, it appears that the effect of this vapour in the vacuum chamber of a mercurial barometer would not be sensible at the ordinary temperatures of the atmosphere, as its tension at 212° F. appears to be equal to only ·001 inch of mercury; and Dr. Prout has allowed me to state, that from his own investigations it appears to have no influence under common circumstances, he having, in summer when the temperature was unusually high, cooled down a mercurial barometer, by means of the evaporation of ether, to 32°, without detecting any such influence, after the requisite correction for the temperature of the mercury itself had been applied. Mr. Snow Harris of Plymouth, having made a variety of experiments on the effects produced on barometers by the introduction of different gases into their vacuum chambers, has kindly offered to furnish me with the detail and results of his experiments, to lay before the Society in connexion with my own.—With regard to the second inquiry, I have compared two excellent and similar mountain barometers, for the use of which I was indebted to Mr. Cary,—first, under the same circumstances and temperature; and afterwards under the same circumstances in every respect excepting the temperature, which in the latter case was considerably raised. The mean difference obtained in one case was not sufficiently unequal to that obtained in the other to indicate any error or discrepancy in the Tables by which the observations were reduced. From Dr. PROUT's experiment also just named, these Tables of Professor SCHUMACHER, which he employed on that occasion, appear to be rigidly correct.—With respect to the third subject of investigation, the influence of the diameter of the tube, I am again indebted to the liberality of Mr. DOLLOND, who has, at his own expense, fitted up for my use a compound barometer, consisting of six tubes of different internal diameters, from 0·13 to 0·50 of an inch, all standing in the same cistern, and the heights read off by an index and scale common to them all. This instrument has already furnished some new and interesting results, and I hope to be able to make, and present to the Society, a complete series of observations by its means.—The fourth subject of inquiry, the connexion between the magnetic and barometrical variation, has been delayed, in consequence of the variation needles with which Mr. DOLLOND intended also to supply me, having, from the peculiarity of their construction, presented unusual anomalies, which he is at present investigating. When these magnetic needles are completed, the series of observations which I propose to make with them, will be rendered more interesting and valuable by the simultaneous observations, both on them and on the barometer, which Captain SMYTH has kindly undertaken to make at his Observatory at Bedford.—The fifth and sixth inquiries involve so many considerations, and require a still so much greater number of observations, that no conclusions can at present be drawn in reference to them: and in the seventh, the comparison of the Baroscope and the use of other instruments, different in principle, but all exhibiting changes in the atmospheric pressure, will be employed.

Among the comparisons which I propose to institute, those with the invaluable observations made at different stations, during the late Captain FOSTER's scientific voyage of discovery in the Chanticleer, by that lamented commander and the officers who accompanied him, and which the President and Council have, at Mr. LUBBOCK's request*, allowed me to make use of for this purpose, will be the first and most important; and their value will be enhanced by the comparison which, through the permission Captain BEAUFORT has kindly granted me, I shall be allowed to make between the Royal Society's Standard, and the Mountain Barometers actually employed in those observations, and which are now deposited at the Admiralty. The barometer also just received from Germany, and made under the direction of Professor SCHUMACHER, at the request of the Royal Society, by BUZENGEIGER of Tübingen; and the barometer now in progress for the Society, under the direction of Dr. PROUT and Professor DANIELL, will, along with observations made both at home and abroad, furnish interesting data for future comparison.

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* The interest taken by Mr. LUBBOCK in my inquiries, the encouragement he has so constantly afforded me in the prosecution of them, and the valuable advice which, on the occurrence of every anomalous result, he has been always so willing to give me, require my best acknowledgement.

In the present communication, I have laid before the Society the results of a classification of my observations according to the place of the sun;—on a future occasion, I propose to add those derived from arrangements made in reference to the position of the moon. I have also in this part of my paper presented data for investigating the constant horary oscillation of the barometer, and I hope to be enabled on a future occasion to submit to the Society the requisite data for examining the diurnal, monthly, and annual variations of that instrument, as well as to deduce results from inquiries made into the laws and nature of the ordinary and inconstant fluctuations exhibited by the mercurial column.

Mr. DANIELL having ascertained the deterioration of barometers in consequence of the insinuation of air between the glass and mercury into the vacuum, it became imperative upon me to ascertain if possible whether the Royal Society's Standard had become injured from this cause, and whether the results obtained from the observations made with it differed practically and in sensible amount, from those made with Mr. DANIELL's Mountain Barometer, an instrument considered by him as almost perfect, or with an instrument like the Water Barometer, widely distinct in its nature and in the corrections required for its reduction. I therefore carefully observed these three instruments simultaneously for 360 successive hours; and their results, already detailed, do not appear to differ essentially from each other in reference to the general accuracy of the Standard Barometer. The variations are nearly the same in amount as those of the Water Barometer, and both these and the mean of the observations, in reference to the Mountain Barometer, appear to be too nearly identical to allow of the supposition of a deterioration to any extent having taken place in the Standard Barometer. The two mercurial barometers give a difference of only ·007 of an inch in mean results derived from these 360 simultaneous observations; and as the Royal Society's Standard is placed at an elevation of forty-two feet above the Mountain Barometer, this small quantity by which it stands lower than the other, does not seem to indicate any of that undue depression of its mercurial column which ought to result from the insinuation of air into its vacuum. The mahogany pillar, also, which forms an intermediate portion of its scale, may be inferred, from the same simultaneous comparison with the Mountain Barometer, which is furnished with a continuous brass scale, as well as from the circumstances of the dimensions of the pillar, the polished surface of its sides, the brass plate on its upper surface, and the careful insertion of its lower end into the cistern of the instrument, not to be subject to the same hygrometric influence as instruments of less guarded construction. I may add that a gentleman, who has been for some time extensively engaged in the prosecution of barometric levelling, determined the elevation of this Standard Barometer above the level of the river, to within a very small extent of the estimated altitude, from the published observations only which had been made with it; and Mr. RICHARDSON, of the Royal Observatory at Greenwich, has informed me, that in an extensive examination of barometrical observations which he was required, for particular astronomical reductions, to make, he found the published observations made with the Standard Barometer of the Royal Society to accord more accurately in their changes with the general result of those, made both in this country and on the Continent, which he had occasion to consult, than any of the other observations he made use of for that purpose.

XXV. *Note on the Tides in the Port of London.* By J. W. LUBBOCK, *Esq. V.P. and Treas. R.S.*

Read June 25, 1832.

MR. STRATFORD has favoured me with a comparison of the predicted times of high water deduced from Mr. BULPIT's Tables, WHITE's Ephemeris, and the British Almanac, with the observations at the London Docks. These observations are, unfortunately, so imperfect, that the differences must not be entirely attributed to the errors of the Tables, which, however, seem susceptible of much improvement. I subjoin this comparison; and in order to convey an idea of the confidence which may be placed in the observations, I also subjoin a comparison, by Mr. DEACON, of the observations at the London and St. Katherine's Docks, which are made according to the same plan, and of which the merit is the same. The differences in the determinations at these two places, which are only about a quarter of a mile distant from each other, may serve to indicate the reliance which can be placed in either.

In my paper on the Tides at Brest, I remarked that the retard or the constant $\lambda - \lambda_{,}$ is considerably greater as deduced from observation here than at Brest. That this must be the case is also evident from the following very simple à priori considerations.—The highest high water takes place when the moon passes the meridian at a time equal to the retard. The tide is propagated from Brest to London, round Scotland, in about twenty-two hours, that is, supposing the tide which takes place in our river to be principally due to that branch of the tide which descends along the eastern coast of Great Britain, which I believe to be the case. The *highest tide* therefore is propagated from Brest to London in about twenty-two hours, and the difference in the retard or in the constant $\lambda - \lambda_{,}$ will be nearly the moon's motion in twenty-two hours, or about 11°; I made the difference in the retard from observation 10°. The tide takes about fifteen hours to reach Brest from the Cape of Good Hope; no doubt the retard there is considerably less.

1832.

Days.		January.			February.			March.		
		B—O	G—O	L—O	B—O	G—O	L—O	B—O	G—O	L—O
		m.	m.	m.	m.	m.	m.	m.	m.	m.
1	M	+3	−20	−18	−17	−33	−18	−14	−35	−11
	A	+10	−11	−13	−13	−24	−11	−23	−40	−19
2	M	+7	−13	−18	+3	−7	+6	−8	−22	−7
	A	+12	−6	−14	+2	−6	+2	−2	−14	−4
3	M	+5	−8	−19	−2	−10	+4	−2	−16	−0
	A	+15	+9	−5	0	−9	+6	−5	−17	−2
4	M	+13	+6	−6	+7	−3	+13	−4	−16	−1
	A	+11	+3	−9	−9	−18	+1	0	−10	+6
5	M	+14	+9	+2	+3	−7	+14	−17	−14	+3
	A	+6	+6	−1	−4	−16	+9	−12	−18	+3
6	M	+1	+1	−5	−7	−18	+7	+10	+7	+25
	A	−5	−5	−5	+6	−4	+23	−13	−4	+11
7	M	−4	−2	−0	−1	−10	+17	−19	−19	−5
	A	−11	+9	−3	−2	−12	+18	+8	+6	+17
8	M	−3	+4	+14	−12	−19	+5	−1	−1	+5
	A	−7	−3	+10	−19	−26	−6	−1	+3	+2
9	M	+2	+7	+20	−5	−10	+6	+5	+11	−1
	A	−9	−3	+12	−15	−19	−9	−4	+4	−16
10	M	−2	+4	+22	+10	+8	+8	−6	−2	−25
	A	−13	−9	+13	−9	−5	−24	−9	+7	−31
11	M	+14	+18	+36	+9	+11	−11	0	+17	−22
	A	+3	+5	+23	−15	−8	−33	−23	+3	−39
12	M	−4	+8	+16	−9	0	−22	−1	+27	−2
	A	+3	+6	+12	−32	−24	−44	−29	−19	−27
13	M	+14	+19	+21	−7	0	−20	−25	−6	−17
	A	−12	−4	−6	......	......	......	−19	......	−15
14	M	−3	+9	+1	......	−11				......
	A	−22	−8	−20	−8	−5	−13	−6	−2	−3
15	M	−12	+4	−12	−1	+2	−6	0	+3	+4
	A	......	......	......	+7	+10	+8	+3	+3	+6
16	M	−21	−4	−27	−7	−1	−3	+2	+4	+8
	A	−63*	−48*	−72*	+3	+6	+7	−5	−3	+1
17	M	−7	−1	−18	+3	+4	+7	+7	+11	+13
	A	−5	+12	−15	+11	+6	+17	+4	+3	+8
18	M	−2	+15	−9	+7	+3	+20	+17	+16	+21
	A	−6	+16	−2	−6	+16	+5	−8	−8	+2
19	M	+18	+28	+13	−12	−19	+5	+2	+3	+15
	A	−6	+11	+4	−25	−35	−10	−6	−6	+7
20	M	−2	+4	+3	−11	−20	+3	+1	0	+11
	A	−11	−1	+5	−9	−23	+2	−2	−1	+6
21	M	−18	−10	0	−9	−25	0	−4	−2	+2
	A	−5	−2	+13	−18	−26	−14	−18	−13	−15
22	M	−7	−9	+12	−10	−26	−8	−6	+1	−7
	A	−9	−15	+11	−11	−24	−14	−5	+8	−5
23	M	−0	−10	+17	−5	−26	−15	−7	+11	−10
	A	−6	−18	+10	−11	−11	−26	−1	+20	−8
24	M	−4	−16	+11	−9	−11	−24	+9	+30	−2
	A	−14	−28	−7	+2	−11	−29	−2	+13	−24
25	M	+4	−10	+11	+17	0	−19	+7	+16	−23
	A	0	−13	+4	+20	+3	−16	−26	+38	−1
26	M	+17	+4	+18	+3	−18	−32	+15	+16	−19
	A	+8	−10	+1	−16	−41	−44	+6	+4	−19
27	M	+5	−14	−3	−13	−39	−30	+19	−27	−35
	A	+5	−23	−9	−20	−57	−32	−11	−21	−20
28	M	+10	−21	−9	......	......	......	−7	−20	−6
	A	+9	−26	−9						
29	M	+10	−25	−6	−12	−37	−13	......	......	−20
	A	......	......	......	−18	−40	−16	−5	−9	−2
30	M	−4	......	−17	......	......	......	0	−14	+2
	A	−4	−31	−12	......	......	......	−6	−18	−7
31	M	−2	−26	−8	......	......	......	−6	−15	−9
	A	−13	−31	−13	......	......	......	−8	−11	−11

* These differences evidently arise from an error in the observation.

This Table contains the results of a comparison of the *predicted* times of high water at the London Docks, with observations of the same, made morning and afternoon, in the months of January, February, and March, 1832.

The *predicted* times have been deduced from Bulpit's Tide Tables, White's Ephemeris, and the British Almanac.

Bulpit's Tables contain the *mean* time of high water at the entrance gate of the East India Docks. The time of high water at the London Docks has been found by adding twenty minutes to the time given by Bulpit.

White's Ephemeris and the British Almanac give the time of high water at London Bridge: these times have been decreased by ten minutes, to obtain the time for the London Docks.

M denotes the *morning* } tides.
A ——— *afternoon* }

B denotes the time of high water at the London Docks, as deduced from Bulpit's Tables.
G do. from White's Ephemeris.
L do. from British Almanac.
O do. as observed.

Wherever blanks occur, it is to be understood that there has not been any tide observed or predicted,—as on the afternoon of Jan. 15; or that the objects of comparison fail, as on the afternoon of Feb. 13, and morning of the 14th, where Bulpit would indicate a tide at $11^h\ 52^m$, which did not in fact occur, and give no tide for the morning of the 14th to compare with an observation on that day.

W. S. S.

June 8, 1832.

	Time.			Height of Tide.			Time.			Height of Tide.		
	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.
	h m	h m	m	ft. in.	ft. in.	ft. in.	h m	h m	m	ft. in.	ft. in.	ft. in.
Jan. 1	0 55	1 10	+15	20 9	25 9	+5 0	1 10	1 15	+ 5	20 8	25 8	+5 0
2	1 35	1 45	+10	21 0	26 1	+5 1	1 50	2 0	+10	21 3	26 5	+5 2
3	2 15	2 15	0	21 8	26 7	+4 11	2 20	2 38	+18	22 0	26 11	+4 11
4	2 40	2 50	+10	22 0	26 11	+4 11	3 0	3 10	+10	22 0	26 11	+4 11
5	3 10	3 20	+10	22 0	27 0	+5 0	3 30	3 43	+13	22 5	27 4	+4 11
6	3 50	4 0	+10	22 4	27 4	+5 0	4 10	4 15	+ 5	22 2	27 0	+4 10
7	4 25	4 30	+ 5	22 3	27 3	+5 0	4 50	4 43	− 7	21 10	26 9	+4 11
8	4 55	4 45	−10	22 0	27 1	+5 1	5 20	5 25	+ 5	21 5	26 3	+4 10
9	5 30	5 38	+ 8	21 2	26 2	+5 0	6 0	6 5	+ 5	21 1	26 2	+5 1
10	6 15	6 13	− 2	21 2	25 5	+4 3	6 50	6 43	− 7	20 9	25 10	+4 11
11	6 50	6 53	+ 3	20 9	25 9	+5 0	7 30	7 38	+ 8	20 8	25 8	+5 0
12	8 0	8 8	+ 8	20 5	25 5	+5 0	8 35	8 45	+10	20 6	25 6	+5 0
13	9 0	9 8	+ 8	20 3	25 2	+4 11	10 0	10 5	+ 5	21 11	26 10	+4 11
14	10 25	10 28	+ 3	20 0	25 0	+5 0	11 20	11 25	+ 5	21 0	26 0	+5 0
15	11 45	11 50	+ 5	20 6	25 4	+4 10						
16	0 50	0 30	−20	21 2	26 1	+4 11	1 45	1 48	+ 3	20 10	26 9	+5 11
17	1 20	1 30	+10	22 2	27 2	+5 0	1 45	1 48	+ 3	22 10	26 10	+4 0
18	2 10	2 10	0	22 9	27 9	+5 0	2 30	2 38	+ 8	23 3	28 2	+4 11
19	2 40	3 5	+25	23 0	28 1	+5 1	3 15	3 33	+18	23 3	28 2	+4 11
20	3 40	3 48	+ 8	23 1	28 0	+4 11	4 5	4 15	+10	23 3	28 2	+4 11
21	4 35	4 35	0	23 0	27 10	+4 10	4 45	5 5	+20	22 7	28 4	+5 9
22	5 10	5 20	+10	22 6	27 6	+5 0	5 35	5 48	+13	22 0	27 0	+5 0
23	5 50	6 3	+13	21 7	26 6	+4 11	6 20	6 38	+18	21 3	26 2	+4 11
24	6 40	6 43	+ 3	20 6	25 6	+5 0	7 15	7 25	+10	19 10	24 8	+4 10
25	7 20	7 18	− 2	19 7	23 11	+4 4	7 50	8 0	+10	20 7	25 6	+4 11
26	8 0	8 6	+ 6	19 8	24 8	+5 0	8 45	8 53	+ 8	19 7	24 6	+4 11
27	9 20	9 25	+ 5	19 11	24 8	+4 9	10 0	10 0	0	18 7	23 7	+5 0
28	10 30	10 53	+23	18 7	23 6	+4 11	11 5	11 18	+13	18 11	23 10	+4 11
29	11 35	11 18	−17	20 7	23 10	+3 3						
30	0 15	0 25	+10	20 4	25 4	+5 0	0 40	0 48	+ 8	19 3	24 3	+5 0
31	1 0	1 10	+10	21 5	26 4	+4 11	1 30	1 35	+ 5	21 1	25 11	+4 10

	Time.			Height of Tide.			Time.			Height of Tide.		
	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.
	h m	h m	m	ft. in.	ft. in.	ft. in.	h m	h m	m	ft. in.	ft. in.	ft. in.
Feb. 1	1 55	1 55	0	21 1	26 1	+5 0	2 10	2 23	+13	21 3	26 3	+5 0
2	2 15	2 15	0	22 3	27 2	+4 11	2 35	2 55	+20	22 6	27 4	+4 10
3	2 55	2 55	0	22 5	27 5	+5 0	3 10	3 20	+10	22 6	27 5	+4 11
4	3 20	3 20	0	22 0	27 0	+5 0	3 50	4 8	+18	20 4	25 3	+4 11
5	3 55	4 0	+ 5	22 7	27 6	+4 11	4 20	4 35	+15	21 7	26 4	+4 9
6	4 40	4 38	− 2	21 8	26 9	+4 11	4 45	5 8	+23	22 6	27 2	+4 8
7	5 10	5 5	− 5	22 1	27 1	+5 0	5 30	5 45	+15	22 4	27 2	+4 10
8	6 0	5 53	− 7	22 0	26 2	+4 2	6 30	6 35	+ 5	20 10	25 9	+4 11
9	6 40	6 40	0	21 0	26 0	+5 0	7 15	7 23	+ 8	20 8	25 8	+5 0
10	7 20	7 30	+10	20 6	25 6	+5 0	8 15	8 23	+ 8	19 11	25 0	+5 1
11	8 30	8 45	+15	19 9	24 8	+4 11	9 30	9 41	+11	19 9	24 9	+5 0
12	10 0	10 15	+15	20 6	25 5	+4 11	11 5	11 5	0	20 9	25 8	+4 11
13	11 20	11 30	+10	20 3	25 3	+5 0						
14	0 10	0 18	+ 8	20 9	25 8	+4 11	0 35	0 43	+ 8	21 3	26 1	+4 10
15	1 0	1 8	+ 8	21 9	26 7	+4 10	1 20	1 40	+20	21 6	27 5	+5 11
16	2 0	2 6	+ 6	22 5	27 5	+5 0	2 15	2 30	+15	23 3	28 1	+4 10
17	2 40	2 53	+13	22 7	27 7	+5 0	2 55	3 20	+25	23 10	28 8	+4 10
18	3 15	3 35	+20	23 4	28 3	+4 11	3 50	4 5	+15	23 3	28 1	+4 10
19	4 10	4 21	+11	22 10	27 9	+4 11	4 45	4 51	+ 6	22 8	27 6	+4 10
20	4 50	5 10	+20	22 0	26 11	+4 11	5 10	5 33	+23	22 0	26 8	+4 8
21	5 30	5 35	+ 5	21 7	26 6	+4 11	6 0	6 5	+ 5	21 6	26 2	+4 8
22	6 10	6 16	+ 6	21 5	26 2	+4 9	6 30	6 35	+ 5	20 5	25 4	+4 11
23	6 45	6 55	+10	19 10	24 8	+4 10	7 15	7 23	+ 8	19 6	24 5	+4 11
24	7 30	7 40	+10	19 2	24 0	+4 10	8 0	7 58	− 2	18 10	23 8	+4 10
25	8 20	8 43	+23	18 2	23 1	+4 11	8 50	9 8	+18	18 7	23 4	+4 9
26	9 45	9 8	−37	18 5	23 5	+5 0	10 40	10 46	+ 6	17 11	22 10	+4 11
27	11 10	11 18	+ 8	18 8	23 8	+5 0	11 50	11 53	+ 3	19 1	24 1	+5 0
28	……	……	……	……	……	………	0 20	0 30	+10	19 7	24 6	+4 11
29	0 40	0 53	+13	20 0	24 11	+4 11	1 10	1 20	+10	20 3	25 2	+4 11

	Time.			Height of Tide.			Time.			Height of Tide.		
	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.	London Docks.	St. Katherine's Docks.	Difference.
	h m	h m	m	ft. in.	ft. in.	ft. in.	h m	h m	m	ft. in.	ft. in.	ft. in.
March 1	1 30	1 35	+ 5	20 2	25 2	+5 0	2 0	1 58	− 2	21 0	25 10	+4 10
2	2 5	2 10	+ 5	21 2	26 2	+5 0	2 20	2 35	+15	21 5	26 5	+5 0
3	2 40	2 46	+ 6	21 4	26 3	+4 11	3 0	3 11	+11	20 10	25 9	+4 11
4	3 15	3 20	+ 5	21 0	25 11	+4 11	3 25	3 30	+ 5	21 6	27 4	+5 10
5	3 45	3 45	0	21 3	26 2	+4 11	4 5	4 14	+ 9	21 5	26 4	+4 11
6	4 0	4 10	+10	22 11	27 10	+4 11	4 30	4 45	+15	19 10	24 8	+4 10
7	5 5	5 15	+10	21 6	26 4	+4 10	5 0	5 8	+ 8	22 11	27 10	+4 11
8	5 30	5 24	− 6	23 1	28 0	+4 11	5 50	6 0	+10	21 11	26 10	+4 11
9	6 10	6 15	+ 5	21 11	26 10	+4 11	6 45	6 48	+ 3	20 10	25 9	+4 11
10	7 15	7 15	0	20 7	25 7	+5 0	7 50	7 55	+ 5	19 8	24 7	+4 11
11	8 15	8 18	+ 3	19 6	24 5	+4 11	9 15	9 18	+ 3	20 0	25 0	+5 0
12	9 30	9 40	+10	19 9	24 8	+4 11	10 45	10 45	0	19 7	27 4	+7 9
13	11 20	11 18	− 2	20 3	25 2	+4 11	12 0	0 5	+ 5	20 6	25 6	+5 0
14	......	... ..	... ..	......	......	......	0 25	0 23	− 2	21 3	26 2	+4 11
15	0 50	1 8	+18	21 6	26 7	+5 1	1 15	1 23	+ 8	23 0	27 10	+4 10
16	1 40	1 50	+10	22 3	27 2	+4 11	2 10	2 13	+ 3	22 0	26 10	+4 10
17	2 20	2 25	+ 5	21 6	26 5	+4 11	2 45	2 55	+10	23 10	28 8	+4 10
18	2 50	2 55	+ 5	23 0	27 9	+4 9	3 30	3 30	0	23 0	28 1	+5 1
19	3 35	3 35	0	24 0	28 11	+4 11	4 0	4 10	+10	21 6	27 4	+5 10
20	4 10	4 18	+ 8	22 2	27 2	+5 0	4 30	4 33	+ 3	23 9	28 9	+5 0
21	4 50	4 56	+ 6	22 0	27 2	+5 2	5 20	5 30	+10	21 6	26 5	+4 11
22	5 25	5 30	+ 5	21 8	26 7	+4 11	5 40	5 55	+15	20 10	25 7	+4 9
23	6 0	6 5	+ 5	20 8	25 7	+4 11	6 15	6 30	+15	20 4	25 4	+5 0
24	6 30	6 40	+10	19 0	23 11	+4 11	7 15	7 20	+ 5	19 1	24 0	+4 11
25	7 40	7 38	− 2	18 6	22 4	+3 10	7 50	7 48	− 2	17 0	21 10	+4 10
26	8 45	8 48	+ 3	17 7	22 5	+4 10	9 30	9 40	+10	17 0	21 11	+4 11
27	10 35	10 40	+ 5	18 10	23 9	+4 11	11 0	11 5	+ 5	18 1	23 0	+4 11
28	11 30	11 48	+18	19 0	23 10	+4 10						
29	0 15	0 10	− 5	19 4	24 4	+5 0	0 30	0 48	+18	21 3	26 1	+4 10
30	0 50	1 2	+12	20 5	25 4	+4 11	1 20	1 32	+12	21 7	26 7	+5 0
31	1 50	1 40	+10	21 8	26 6	+4 10	2 0	2 5	+ 5	22 3	27 2	+4 11

XXVI. *Researches in Physical Astronomy.* By J. W. Lubbock, *Esq. V.P. and Treas. R.S.*

Read June 21, 1832.

On the development of R.

In the following method of developing the disturbing function, the coefficients of the inequalities corresponding to any given order are expressed in terms of the coefficients of the inferior orders; so that, for example, the coefficients of the terms in the disturbing function multiplied by the squares of the eccentricities, are given analytically by means of the coefficients of those independent of the eccentricities, and of those multiplied by their first powers. As the theorems to which this method gives rise, are of great simplicity, I trust they will not be thought unworthy attention. By their means and with the assistance of the table given in my Lunar Theory, the expressions may be obtained, which are necessary for the development of R, as far as the fourth powers of the eccentricities inclusive; it may easily be carried to any extent, and the expressions given by **Burckhardt** in the Mémoires de l'Institut, 1808, may be verified without difficulty. This method is peculiarly advantageous in the lunar theory, and for the terms in R dependent on powers of the eccentricities above the squares; for the expression thus obtained for the coefficients of the terms dependent on the squares and products of the eccentricities in the planetary theory, is by no means so simple or so convenient for numerical calculation as that given in the Phil. Trans. 1831, p. 295. A similar method is applicable to the terms dependent on the inclinations.

Let $R = R_0 + e^2 R_0' + e_{\prime}^2 R_0'' + \&c.$

$$+ \{R_1 + e^2 R_1' + e_{\prime}^2 R_1'' + \&c.\} \cos (i n t - i n_{\prime} t) \qquad [1]$$

$$+ \{R_2 + e^2 R_2' + e_{\prime}^2 R_2'' + \&c.\} e \cos (n t - \varpi) \qquad [2]$$

$$+ \{R_3 + e^2 R_3 + e_1^2 R_3 + \&c\}\, e \cos (int - in_1 t - nt + \varpi)$$

[3]

$$+ \&c.$$

where the indices are as follows, and the same as in my Lunar Theory, merely writing the indeterminate *i* instead of the number 2.

0	0	21	$it - 3x$	42	$it - 3x - z$
1	it	22	$it + 3x$	43	$it + 3x + z$
2	x	23	$2x + z$	44	$3x - z$
3	$it - x$	24	$it - 2x - z$	45	$it - 3x + z$
4	$it + x$	25	$it + 2x + z$	46	$it + 3x - z$
5	z	26	$2x - z$	47	$2x + 2z$
6	$it - z$	27	$it - 2x + z$	48	$it - 2x - 2z$
7	$it + z$	28	$it + 2x - z$	49	$it + 2x + 2z$
8	$2x$	29	$x + 2z$	50	$2x - 2z$
9	$it - 2x$	30	$it - x - 2z$	51	$it - 2x + 2z$
10	$it + 2x$	31	$it + x + 2z$	52	$it + 2x - 2z$
11	$x + z$	32	$x - 2z$	53	$x + 3z$
12	$it - x - z$	33	$it - x + 2z$	54	$it - x - 3z$
13	$it + x + z$	34	$it + x - 2z$	55	$it + x + 3z$
14	$x - z$	35	$3z$	56	$x - 3z$
15	$it - x - z$	36	$it - 3z$	57	$it - x + 3z$
16	$it + x - z$	37	$it + 3z$	58	$it + x - 3z$
17	$2z$	38	$4x$	59	$4z$
18	$it - 2z$	39	$it - 4x$	60	$it - 4z$
19	$it + 2z$	40	$it + 4x$	61	$it + 4z$
20	$3x$	41	$3x + z$		

$$r = 1 + \frac{e^2}{2} - e\left(1 - \frac{3e^2}{8}\right)\cos x - \frac{e^2}{2}\left(1 - \frac{2e^2}{3}\right)\cos 2x + \frac{9}{8}e^3\cos 3x + \frac{4}{3}e^4\cos 4x$$

$$\frac{dr}{de} = e - \left(1 - \frac{9}{8}e^2\right)\cos x - e\left(1 - \frac{4e^2}{3}\right)\cos 2x + \frac{27}{8}e^2\cos 3x + \frac{16}{3}e^3\cos 4x$$

$$\frac{dr}{r\,de} = \frac{e}{2}\left(1 + \frac{e^2}{4}\right) - \left(1 - \frac{9}{8}e^2\right)\cos x - \frac{3}{2}e\left(1 - \frac{11}{9}e^2\right)\cos 2x$$

[0] [2] [8]

$$- \frac{17}{8}e^2\cos 3x - \frac{71}{24}e^3\cos 4x$$

[20] [35]

$$\frac{d\lambda}{de} = 2\left(1 - \frac{3e^2}{8}\right)\sin x + \frac{5}{2}e\left(1 - \frac{28}{15}e^2\right)\sin 2x + \frac{13}{4}e^2\sin 3x + \frac{103}{24}e^2\sin 4x$$

[2] [8] [20] [35]

$$\frac{dR}{de} = \frac{dR}{dr}\frac{dr}{de} + \frac{dR}{d\lambda}\frac{d\lambda}{de}$$

$$= \frac{r\,dR}{dr}\frac{dr}{r\,de} + \frac{dR}{d\lambda}\frac{d\lambda}{de}$$

$$\frac{r\,\mathrm{d}R}{\mathrm{d}r}=\frac{a\,\mathrm{d}R}{\mathrm{d}a} \qquad \frac{\mathrm{d}R}{\mathrm{d}\lambda}=-iR^{*}$$

Multiplying by means of Table II. Phil. Trans. 1831, p. 238, we find

$$R_2=-\frac{a\,\mathrm{d}R_0}{\mathrm{d}a} \qquad R_3=-\frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a}-i\,R_1 \qquad R_4=-\frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a}+iR_1$$

$$2\,R_8=-\frac{a\,\mathrm{d}R_2}{2\,\mathrm{d}a}-\frac{3\,a\,\mathrm{d}R_0}{2\,\mathrm{d}a}$$

$$2\,R_9=-\frac{a\,\mathrm{d}R_3}{2\,\mathrm{d}a}-i\,R_3-\frac{3\,a\,\mathrm{d}R_1}{4\,\mathrm{d}a}-\frac{5\,i\,R_1}{4}$$

$$2\,R_{10}=-\frac{a\,\mathrm{d}R_4}{2\,\mathrm{d}a}+i\,R_4-\frac{3\,a\,\mathrm{d}R_1}{4\,\mathrm{d}a}+\frac{5\,i\,R_1}{4}$$

These equations may be formed at once from the Table by inspection, taking care to write R with the sign + in the term multiplied by i when the index is found in the upper line in the Table, as in the case of the argument (10); and with the sign − when in the lower, as in the case of the argument (9). The term multiplied by $\frac{a\,\mathrm{d}R}{\mathrm{d}a}$ always takes its sign from the factor arising from $\frac{\mathrm{d}r}{r\,\mathrm{d}e}$. In what precedes, i is any positive whole number.

By means of the Tables, any term in R depending on the eccentricities may be found at pleasure, and the development given in the Phil. Trans. 1831, p. 263, may be verified with great facility; thus

$$4\,R_{38}=-\frac{a\,\mathrm{d}R_{20}}{2\,\mathrm{d}a}-\frac{3\,a\,\mathrm{d}R_8}{4\,\mathrm{d}a}-\frac{17\,a\,\mathrm{d}R_2}{16\,\mathrm{d}a}-\frac{71\,a\,\mathrm{d}R_0}{24\,\mathrm{d}a}$$

I find on reference to the development in question

$$R_{38}=\frac{a^2}{24\,a_{,}^3} \qquad R_{20}=\frac{a^2}{16\,a_{,}^3} \qquad R_8=\frac{a^2}{8\,a_{,}^3} \qquad R_2=\frac{a^2}{2\,a_{,}^3} \qquad R_0=-\frac{a^2}{4\,a_{,}^3}$$

whence

$$a\frac{\mathrm{d}R_{20}}{\mathrm{d}a}=\frac{a^2}{8\,a_{,}^3} \qquad \frac{a\,\mathrm{d}R_8}{\mathrm{d}a}=\frac{a^2}{4\,a_{,}^3} \qquad \frac{a\,\mathrm{d}R_2}{\mathrm{d}a}=\frac{a^2}{a_{,}^3} \qquad \frac{a\,\mathrm{d}R_0}{\mathrm{d}a}=-\frac{a^2}{2\,a_{,}^3}$$

which values satisfy the equation above, for

$$\frac{4}{24}=\frac{1}{2\cdot 8}-\frac{3}{4\cdot 4}-\frac{17}{16}+\frac{71}{24\cdot 2}$$

By successive substitutions in the expressions which have been given, it is obvious that they may be reduced so as to contain only the quantity R_1 and the differential coefficients of this quantity with respect to a and a_i.

---

* This is only a method of notation as regards the coefficients, which will be easily understood.

Thus

$$R_4 = -\frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a} + i R_1$$

$$2R_{10} = -\frac{a\,\mathrm{d}R_4}{2\,\mathrm{d}a} + iR_4 - \frac{3\,a\,\mathrm{d}R_1}{4\,\mathrm{d}a} + \frac{5\,i\,R_1}{4}$$

$$= -\frac{1}{2}\left\{-\frac{a^2\,\mathrm{d}^2R_1}{2\,\mathrm{d}a^2} - \frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a} + i\,a\frac{\mathrm{d}R_1}{\mathrm{d}a}\right\}$$

$$-\frac{i\,a\,\mathrm{d}R_1}{2\,\mathrm{d}a} + i^2R_1 - \frac{3\,a\,\mathrm{d}R_1}{4\,\mathrm{d}a} + \frac{5\,i\,R_1}{4}$$

$$R_{10} = \frac{a^2\,\mathrm{d}^2R_1}{8\,\mathrm{d}a^2} - \frac{(2\,i+1)}{4}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{(4i^2+5\,i)\,R_1}{8}$$

Changing the sign of *i*, we get

$$R_9 = \frac{a^2\,\mathrm{d}^2R_1}{8\,\mathrm{d}a^2} + \frac{(2\,i-1)}{4}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{(4i^2-5\,i)\,R_1}{8}$$

which accords with the expression (for $N^{(0)}$) given in the *Théor. Anal.* vol. i. p. 463.

$$3R_{22} = -\frac{a\,\mathrm{d}R_{10}}{\mathrm{d}a} + i\,R_{10} - \frac{3}{4}\,\frac{a\,\mathrm{d}R_4}{\mathrm{d}a} + \frac{5\,i}{4}R_4 - \frac{17}{16}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{13\,i\,R_1}{8}$$

$$= -\frac{1}{2}\left\{\frac{a^2\,\mathrm{d}^3R_1}{8\,\mathrm{d}a^3} + \frac{a^2\,\mathrm{d}^2R_1}{4\,\mathrm{d}a^2} - \frac{(2\,i+1)}{4}\frac{a^2\,\mathrm{d}^2R_1}{\mathrm{d}a^2} - \frac{(2\,i+1)}{4}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{(4\,i+5)}{8}\frac{i\,\mathrm{d}R_1}{\mathrm{d}a}\right\}$$

$$+\,i\left\{\frac{a^2\,\mathrm{d}R_1}{8\,\mathrm{d}a^2} - \frac{(2\,i+1)}{4}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{(4\,i+5)\,i\,R_1}{8}\right\}$$

$$-\frac{3}{4}\left\{-\frac{a^2\,\mathrm{d}^2R_1}{2\,\mathrm{d}a^2} - \frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a} + \frac{i\,a\,\mathrm{d}R_1}{\mathrm{d}a}\right\}$$

$$+\frac{5\,i}{4}\left\{-\frac{a\,\mathrm{d}R_1}{2\,\mathrm{d}a} + i\,R_1\right\} - \frac{17}{16}\frac{a\,\mathrm{d}R_1}{\mathrm{d}a} + \frac{13}{8}R_1$$

$$R_{22} = \frac{1}{48}\left\{(26\,i + 30\,i^2 + 8\,i^3)\,R_1 - (9 + 27\,i + 12\,i^2)\,\frac{a\,\mathrm{d}R_1}{\mathrm{d}a}\right.$$

$$\left. + (6\,i+6)\,\frac{a^2\,\mathrm{d}^2R_1}{\mathrm{d}a^2} - \frac{a^3\,\mathrm{d}^3R_1}{\mathrm{d}a^3}\right\}$$

Changing the sign of *i*, we get

$$R_{21} = -\frac{1}{48}\left\{(26\,i - 30\,i^2 + 8\,i^3)\,R_1 + (9 - 27\,i + 12\,i^2)\,a\,\frac{\mathrm{d}R}{\mathrm{d}a}\right.$$

$$\left. + (6\,i - 6)\,\frac{a^2\,\mathrm{d}^2 R_1}{\mathrm{d}a^2} + \frac{a^3\,\mathrm{d}^3 R_1}{\mathrm{d}a^3}\right\}$$

which agrees with the expression given by **Burckhardt** for ($M^{(0)}$), *Memoires de l'Institut*, 1808, *Second Semestre*, p. 39.

Similarly

$$2\,R_{51} = \frac{a^2\,\mathrm{d}^2 R_{19}}{4\,\mathrm{d}a^2} + \frac{(2\,i-1)}{2}\,\frac{a\,\mathrm{d}R_{19}}{\mathrm{d}a} + \frac{(4\,i^2 - 5\,i)\,R_{19}}{4}$$

$$2\,R_{19} = \frac{a_{,}^2\,\mathrm{d}^2 R_1}{4\,\mathrm{d}a_{,}^2} + \frac{(2\,i-1)}{2}\,\frac{a_{,}\,\mathrm{d}R_1}{\mathrm{d}a_{,}} + \frac{(4\,i^2 - 5\,i)\,R_1}{4}$$

If $i = 2$,

$$2\,R_{51} = \frac{a^2\,\mathrm{d}^2 R_{19}}{4\,\mathrm{d}a^2} + \frac{3}{2}\,\frac{a\,\mathrm{d}R_{19}}{\mathrm{d}a} + \frac{3}{2}\,R_{19}$$

$$2\,R_{19} = \frac{a_{,}^2\,\mathrm{d}^2 R_1}{4\,\mathrm{d}a_{,}^2} + \frac{3}{2}\,\frac{a_{,}\,\mathrm{d}R_1}{\mathrm{d}a_{,}} + \frac{3}{2}\,R_1$$

$$R_1 = -\frac{b_{1,2}}{a_{,}} = -\frac{3\,a^2}{4\,a_{,}^3} - \frac{3\,.\,5}{2\,.\,4\,.\,6}\,\frac{a^4}{a_{,}^5} - \frac{3\,.\,3\,.\,5\,.\,7}{2\,.\,4\,.\,4\,.\,6\,.\,8}\,\frac{a^6}{a_{,}^7} - \&c.$$

In the Lunar Theory, the higher terms may be neglected; and taking $R_1 = -\frac{3\,a^2}{4\,a_{,}^3}$, it is evident that R_{19} and R_{51} are each equal to zero. This theorem, however, cannot be extended to the other terms, and therefore in the Planetary Theory the coefficient corresponding to the argument $2\,t - 2\,x + 2\,z$ or $2\,\varpi - 2\,\varpi_{,}$, in the development of R, (which term is important as regards the secular inequalities,) does not vanish.

If the coefficients of the nth argument in the expressions for $\frac{a}{r}$ and λ be called r_n and λ_n, the Table which has been used for the preceding multiplications may also be used (when the square of the disturbing force is neglected,) for the integration of the equations

$$\frac{\mathrm{d}^2\,.\,r^2}{2\,\mathrm{d}t^2} - \frac{\mu}{r} + \frac{\mu}{a} + 2\int \mathrm{d}R + r\,\frac{\mathrm{d}R}{\mathrm{d}r} = 0$$

and

$$\frac{\mathrm{d}\lambda}{\mathrm{d}t} = \frac{h}{r^2} - \frac{1}{r^2}\int \frac{\mathrm{d}R}{\mathrm{d}\lambda}\,\mathrm{d}t$$

$$-\frac{\mathrm{d}^2 r^3\,\delta\,\frac{1}{r}}{\mathrm{d}t^2} - \mu\,\delta\,\frac{1}{r} + 2\int \mathrm{d}R + \frac{r\,\mathrm{d}R}{\mathrm{d}r} =$$

$$\frac{r^3}{a^3}=1+3e^2\left(1+\frac{e^2}{8}\right)-3e\left(1+\frac{3}{8}e^2\right)\cos x-\frac{5}{8}e^4\cos 2x+\frac{e^3}{8}\cos 3x+\frac{e^4}{8}\cos 4x$$

[0] [2] [8] [20] [35]

Thus

$$\left\{i(n-n_{,})+3n\right\}^2\left\{(1+3e^2)r_{22}-\frac{3}{2}r_{10}-\frac{r_1}{16}\right\}-r_{22}+\frac{2(in+3n)a}{i(n-n_{,})+3n}R_{22}+\frac{a^2\,\mathrm{d}R_{22}}{\mathrm{d}a}=0$$

If R be considered as a function of $r^{\backprime}$, $\lambda^{\backprime}$ and s, we have

$$\frac{\mathrm{d}R}{\mathrm{d}\gamma}=\frac{r^{\backprime}\mathrm{d}R}{\mathrm{d}r^{\backprime}}\frac{\mathrm{d}r^{\backprime}}{r^{\backprime}\mathrm{d}\gamma}+\frac{\mathrm{d}R}{\mathrm{d}\lambda^{\backprime}}\frac{\mathrm{d}\lambda^{\backprime}}{\mathrm{d}\gamma}+\frac{\mathrm{d}R}{\mathrm{d}s}\frac{\mathrm{d}s}{\mathrm{d}\gamma}$$

$$\frac{\mathrm{d}r^{\backprime}}{r^{\backprime}\mathrm{d}\gamma}=\frac{\gamma}{2}-\frac{\gamma}{2}(1-4e^2)\cos 2y+\gamma e\cos(x-2y)-\gamma e\cos(x+2y)$$

[62] [65] [66]

$$-\frac{3}{8}\gamma e^2\cos(2x-2y)+\frac{13}{8}\gamma e^2\cos(2x+2y)$$

[77] [78]

$$\frac{\mathrm{d}\lambda^{\backprime}}{\mathrm{d}\gamma}=-\gamma(1-4e^2)\sin 2y+\gamma e\sin(x-2y)-3\gamma e\sin(x+2y)$$

[62] [65] [66]

$$-\frac{13}{2}\gamma e^2\sin(2x+2y)$$

[78]

$$\frac{\mathrm{d}s}{\mathrm{d}\gamma}=(1-e^2)\sin y+e\sin(x-y)+e\sin(x+y)+\frac{e^2}{8}\sin(2x-y)+\frac{9}{8}e^2\sin(2x+y)$$

[146] [149] [150] [161] [162]

If R be considered as a function of r, $\lambda^{\backprime}$ and s,

$$\frac{\mathrm{d}R}{\mathrm{d}\gamma}=\frac{\mathrm{d}R}{\mathrm{d}\lambda^{\backprime}}\frac{\mathrm{d}\lambda^{\backprime}}{\mathrm{d}\gamma}+\frac{\mathrm{d}R}{\mathrm{d}s}\frac{\mathrm{d}s}{\mathrm{d}\gamma}$$

$\frac{\mathrm{d}R}{\mathrm{d}\lambda^{\backprime}}=-iR$ as before, and the expression for $\frac{\mathrm{d}R}{\mathrm{d}s}$ (in this case) is given for the Lunar Theory, Phil. Trans. 1832, p. 6.

The multiplications required may be effected by means of the following Table. In the terms multiplied by $\frac{\mathrm{d}\lambda^{\backprime}}{\mathrm{d}\gamma}$, the coefficient of R is to be taken with a positive sign when its index is found in the upper line, and with a negative in the contrary case. In the terms multiplied by $\frac{\mathrm{d}R}{\mathrm{d}s}$, the coefficient of $\frac{\mathrm{d}R}{\mathrm{d}s}$ is to be taken with a negative sign when the index is found in the upper line, and with a positive when in the lower.

	62	65	66	77	78	146	149	150	161	162	
62	... ...	... 2	... −2	... ...	... ...	146	150	...... −149	....	....	62
63	... 1	3 ..	... 4	... ...	... ...	 147	151	 153	....	....	63
64	1 ...	... 4	3 ...	... ...	... ...	148	 154	152	....	....	64
65	... 2	... ...	... ...	... ...	... ...	 149	...... −146	....	....	....	65
66	2 ...	... ...	... ...	... ...	... ...	150	....	146	....	....	66
67	... 3	... ...	... 1	... ...	... ...	 151	....	 147	....	....	67
68	3 ...	... 1	... ...	... ...	... ...	152	 148	....	....	....	68
69	... 4	1 ...	... ...	... ...	... ...	 153	147	....	....	....	69
70	4 ...	... ...	1 ...	... ...	... ...	154	....	148	....	....	70
71	... 5	... ...	... ...	... ...	... ...	 155	....	....	....	....	71
72	5 ...	... ...	... ...	... ...	... ...	156	....	....	....	....	72
73	... 6	... ...	... ...	... ...	... ...	 157	....	....	....	....	73
74	6 ...	... ...	... ...	... ...	... ...	158	....	....	....	....	74
75	... 7	... ...	... ...	... ...	... ...	 159	....	....	....	....	75
76	7 ...	... ...	... ...	... ...	... ...	160	....	....	....	....	76
77	... 8	2 ...	... ...	... ...	... ...	 161	149	....	...... −146	....	77
78	8 ...	... ...	2 ...	... ...	... ...	162	....	150	....	146	78
79	... 9	... ...	.. 3	... ...	... 1	 163	....	 151	....	 147	79
80	9 ...	... 3	... ...	... 1	... ...	164	 152	....	 148	....	80
81	... 10	4 ...	... ...	1 ...	... ...	 165	153	....	147	....	81
82	10 ...	... ...	4 ...	... ...	1 ...	166	....	154	....	148	82
83	... 11	5 ...	... ...	... ...	... ...	 167	155	....	....	....	83
84	11 ...	... ...	5 ...	... ...	... ...	168	....	156	....	....	84
85	... 12	... ...	... 6	... ...	... ...	 169	....	 157	....	....	85
86	12 ...	... 6	... ...	... ...	... ...	170	 158	....	....	....	86
87	... 13	7 ...	... ...	... ...	... ...	 171	159	....	....	....	87
88	13 ...	... ...	7 ...	... ...	... ...	172	....	160	....	....	88
89	... 14	.. −5	... ...	... ...	... ...	 173	...... −156	....	....	....	89
90	14 ...	... ...	... −5	... ...	... ...	174	....	...... −155	....	....	90
91	... 15	... ...	... 7	... ...	... ...	 175	....	 159	....	....	91
92	15 ...	... 7	... ...	... ...	... ...	176	 160	....	....	....	92
93	... 16	6 ...	... ...	... ...	... ...	 177	157	....	....	....	93
94	16 ...	... ...	6 ...	... ...	... ...	178	....	158	....	....	94

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AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

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1832. January.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
☉ 1	30.215	36.2	30.172	37.3	26	30.6	34.3	28.3	34.3		NNE	Overcast. A.M. Fog and hoar frost.
☽ 2	29.978	35.7	29.867	37.2	32	32.8	34.8	28.7	34.8		E	Overcast—light wind.
● ♂ 3	29.829	36.6	29.825	37.5	30	32.0	33.4	30.7	33.4		SSW	Foggy.
☿ 4	29.696	35.2	29.657	36.2	28	29.8	33.5	28.3	33.5		SSW	Foggy and cloudy.
♃ 5	29.650	34.3	29.609	35.0	26	28.7	31.3	27.8	34.7		ESE	Overcast—light wind.
♀ 6	29.517	36.3	29.439	37.6	34	35.7	37.4	37.8	38.0		E	Overcast—light wind.
♄ 7	29.361	38.7	29.340	41.0	35	36.7	40.2	35.4	40.2		E	Lightly overcast—light fog and wind.
☉ 8	29.386	41.3	29.390	42.3	37	37.4	40.3	36.4	40.3	0.069	E	A.M. Fog and deposition. P.M. Fair—light wind.
☽ 9	29.497	42.0	29.425	42.6	38	40.4	41.3	36.7	46.6		ESE	A.M. Fog. P.M. Light rain.
♂ 10	29.608	44.7	29.651	46.6	44	45.4	48.3	39.7	48.7		WSW	A.M. Fair—light wind. P.M. Light rain.
☿ 11	29.792	45.7	29.669	47.4	44	44.5	47.2	43.5	47.3	0.100	SSW	Fair—lightly cloudy.
♃ 12	29.828	45.6	29.728	47.2	42	42.4	45.8	40.6	45.8	0.153	SSW	Light clouds and wind.
♀ 13	29.586	46.4	29.624	47.2	41	41.3	42.2	40.7	42.2	0.278	W	A.M. Foggy. P.M. Light rain and wind.
♄ 14	30.135	41.5	30.168	42.7	35	35.0	38.5	32.8	38.5		NNW	Fair—lightly overcast.
☉ 15	30.459	39.7	30.471	41.4	26	32.7	35.8	31.4	35.8		NNE	A.M. Overcast—hoar frost. P.M. Fair—light wind.
☽ 16	30.472	37.7	30.414	39.6	25	31.4	36.4	28.7	37.7		SW	Lightly overcast—light fog and hoar frost.
○ ♂ 17	30.334	40.5	30.313	41.7	36	39.2	41.7	29.9	41.7		W	Overcast—fog.
☿ 18	30.358	43.3	30.353	42.8	37	37.7	41.0	37.3	41.0		W	Lightly overcast.
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♀ 20	30.185	42.5	30.131	42.7	30	32.3	39.8	30.6	39.8		S	Overcast—light fog.
♄ 21	30.209	42.6	30.211	44.2	36	38.3	43.3	31.6	43.3		SSW	Lightly overcast. A.M. Light fog and deposition.
☉ 22	30.246	44.7	30.232	45.4	41	41.7	41.7	37.7	42.7		S	Lightly cloudy.
☽ 23	30.311	45.2	30.316	45.4	40	40.8	42.3	40.4	42.3		SSE	Overcast—deposition.
♂ 24	30.234	43.4	30.125	45.1	38	38.7	44.6	34.6	44.7		S	Fine and cloudless—light haze and fog. Unsteady wind at night.
☿ 25	29.834	45.7	29.833	46.9	43	45.4	45.3	38.3	46.4		SSW	A.M. Rain. P.M. Fair—light fog.
♃ 26	29.859	45.7	29.810	46.3	33	36.7	42.3	34.6	42.3		WSW	Fine—light clouds and haze.
♀ 27	29.934	44.7	30.040	43.8	34	35.3	38.7	34.2	38.7	0.055	N	A.M. Rain and snow. P.M. Fair.
♄ 28	30.262	41.7	30.207	41.7	26	31.8	37.5	29.8	39.8		NNW	Lightly overcast.
☉ 29	30.203	42.7	30.278	44.3	39	41.0	45.5	31.1	45.5	0.014	W	Foggy—deposition.
☽ 30	30.326	43.9	30.234	44.8	37	40.3	43.7	38.8	43.7		W	Overcast—light fog.
♂ 31	30.084	45.3	29.959	45.7	34	45.5	43.3	39.7	45.5		WSW	Lightly overcast.
	Mean 29.991	Mean 41.7	Mean 29.961	Mean 42.6	Mean 34.9	Mean 37.4	Mean 40.2	Mean 34.5	Mean 40.8	Sum 0.669		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr.	9 A.M.	3 P.M.
	29.960	29.927

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2¼ in.

.... above the mean level of the Sea (presumed about).............. =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR FEBRUARY, 1832.

1832. February.	9 o'clock, A.M.		3 o'clock, P.M.		Dew point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
● ☿ 1	29.481	43.2	29.299	43.3	32	36.3	41.6	35.4	44.5		ESE	A.M. Cloudy. P.M. Fair—light wind.
♃ 2	29.227	44.8	29.182	46.3	44	45.3	44.4	35.6	46.3		SSW	Light rain and wind.
♀ 3	29.524	43.4	29.693	45.2	35	35.4	42.8	34.3	46.7		SW	Fine and clear.
♄ 4	29.839	46.4	29.900	49.0	43	47.4	51.4	34.7	51.7		SSW	Fine—lightly cloudy. Rain early A.M.
☉ 5	30.021	49.4	30.022	51.4	48	49.9	51.4	46.6	52.7		SSE	Cloudy—light wind. Rain, with high wind, at night.
☽ 6	29.815	50.3	29.692	52.6	44	47.4	47.4	45.8	51.3		SSE	A.M. Fine. P.M. Rain.
♂ 7	29.954	47.7	30.075	49.6	37	40.2	45.5	38.7	45.5		WNW	Cloudless—hazy.
☿ 8	30.339	44.9	30.368	48.6	35	39.3	47.0	35.3	47.0		W	A.M. Foggy. P.M. Fine and clear—light clouds and wind.
♃ 9	30.389	47.3	30.395	49.4	43	45.2	49.6	38.7	49.6		SW	Overcast.—Light fog, with rain, A.M.
♀ 10	30.556	45.5	30.519	47.3	33	37.4	43.7	35.5	43.7	0.069	NNW	A.M. Overcast and hazy. P.M. Fine.
♄ 11	30.349	44.2	30.239	45.7	35	38.2	42.1	36.6	41.6		NNE	A.M. Fine—hazy. P.M. Overcast.
☉ 12	30.152	41.8	30.110	43.7	35	36.7	40.6	34.2	40.7		N	A.M. Fine. P.M. Overcast—rain.
☽ 13	30.087	42.0	30.071	43.2	35	38.5	40.2	36.3	40.2		ENE	Overcast.
♂ 14	30.062	41.3	30.031	42.7	30	36.6	37.9	35.7	37.9		E	A.M. Overcast. P.M. Fair.
☿ 15	30.054	38.7	30.009	39.7	26	30.9	34.3	28.6	34.3		N	A.M. Foggy. P.M. Fine—hazy.
○ ♃ 16	29.834	36.7	29.721	39.7	27	31.8	37.9	28.3	37.9		N	Overcast—hazy.
♀ 17	29.731	40.3	29.845	42.8	35	37.8	43.0	31.0	43.0		WSW	A.M. Fog and deposition. P.M. Light clouds and haze.
♄ 18	30.196	42.2	30.246	44.6	38	40.2	44.7	37.3	44.7		N	Lightly cloudy—haze and light wind.
☉ 19	30.314	41.9	30.271	43.5	32	37.3	39.8	36.3	39.8		N	A.M. Overcast—light fog. P.M. Fine—light wind.
☽ 20	30.333	39.7	30.315	42.9	32	34.7	42.5	30.6	42.5		E	A.M. Hazy. P.M. Fine and cloudless.
♂ 21	30.243	40.7	30.198	43.2	37	37.6	44.8	34.2	44.8		NNE	Fine—hazy.
☿ 22	30.340	39.3	30.342	41.2	32	33.1	37.8	30.7	37.8		NE	Fog and deposition.
♃ 23	30.372	39.4	30.293	40.0	30	32.6	38.2	31.6	38.2		NE	Foggy.
♀ 24	30.152	38.7	30.035	40.3	30	31.0	37.2	29.8	37.2		NNE	Overcast—hazy.—Very dense fog at night.
♄ 25	30.044	37.7	30.117	39.4	32	32.3	36.2	28.3	37.2		NNE	Hazy—light wind.
☉ 26	30.189	38.7	30.129	41.3	36	37.7	41.8	31.3	41.8		NE	A.M. Overcast. P.M. Fine.
☽ 27	30.123	39.8	30.124	40.9	36	36.1	39.6	35.3	39.6		N	Overcast and foggy.
♂ 28	30.205	40.3	30.191	40.6	35	35.8	35.9	34.5	36.7		N	Overcast—light wind.
☿ 29	30.203	39.7	30.190	41.0	29	36.4	39.2	34.3	39.2		NE	Light clouds and wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.073	42.3	30.056	44.1	35.0	37.9	42.0	34.7	42.6	0.069		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... { 9 A.M. 30.041 3 P.M. 30.019 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge .... =83 feet 2½ in.

.... above the mean level of the Sea (presumed about) .... =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided iuto inches and decimals.

METEOROLOGICAL JOURNAL FOR MARCH, 1832.

1832. March.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
♃ 1	30.287	40.7	30.286	42.4	32	38.4	41.8	35.3	41.8		S	Overcast—light wind.
● ♀ 2	30.334	43.9	30.334	47.2	39	41.8	45.8	38.3	45.4		SW	Lightly cloudy.
♄ 3	30.315	43.7	30.229	46.7	37	38.8	43.6	36.4	44.3		SSE	A.M. Overcast. P.M. Fine.
☉ 4	30.000	45.5	29.874	47.7	43	45.2	47.0	38.3	47.4		S	Lowering—light brisk wind. Rain at night.
☽ 5	29.811	46.4	29.917	48.7	40	42.9	46.8	39.1	46.8	0.055	SW	Fine—nearly cloudless—light wind.
♂ 6	29.753	44.3	29.398	46.4	41	42.3	45.3	33.8	46.7		S var.	A.M. Broken clouds, with unsteady wind. P.M. Light rain, with brisk wind.
☿ 7	29.399	45.4	29.330	47.4	40	41.2	44.0	35.4	44.0		S	Fine—nearly cloudless—faint haze and wind.
♃ 8	29.517	42.7	29.645	44.2	33	35.2	39.3	33.3	39.3		NNW	Overcast—light haze and wind.
♀ 9	30.045	42.5	30.158	46.0	33	35.1	45.8	31.7	45.8		NNW	Fine—nearly cloudless. Faint haze and hoar frost A.M.
♄ 10	30.402	41.8	30.316	44.2	30	32.3	43.0	30.7	43.0		?	A.M. Strong haze. P.M. Fair.
☉ 11	30.492	41.3	30.105	42.7	32	35.0	38.7	31.6	38.7		E	Overcast.
☽ 12	30.033	41.7	29.962	44.3	35	36.8	44.5	34.4	44.5		ESE	Overcast.
♂ 13	29.908	42.6	29.819	45.6	35	38.8	45.8	34.7	45.8		WSW	A.M. Overcast. P.M. Fair—light clouds and wind.
☿ 14	29.509	45.8	29.457	48.5	43	45.3	48.2	38.4	49.4		S	Lightly cloudy.—P.M. Light rain.
♃ 15	29.412	44.4	29.580	46.0	41	41.6	44.4	36.7	44.4	0.158	NNE	Light rain, with wind.
○ ♀ 16	29.829	43.7	29.742	46.9	37	36.7	46.2	32.7	50.3		WSW	Overcast.—A.M. Light fog. P.M. Rain.
♄ 17	29.427	48.2	29.463	51.0	50	50.8	50.5	36.4	52.7	0.041	WSW	A.M. Overcast—deposition. P.M. Fair
☉ 18	29.540	48.3	29.501	50.4	42	45.6	46.8	38.7	49.5		WSW	Fair—light clouds. Light shower P.M.
☽ 19	29.871	46.9	29.792	50.4	40	43.8	50.3	38.2	50.6		SW	Overcast—light rain and wind.
♂ 20	29.566	49.4	29.715	51.0	41	46.8	50.3	43.4	50.3		NW	Fine—lightly cloudy—light unsteady wind.
☿ 21	30.047	48.6	30.109	51.6	46	48.8	54.2	40.3	54.4		WSW	Overcast—light fog.
♃ 22	30.165	49.8	30.120	53.6	47	47.8	54.6	43.4	55.3		WSW	Lightly cloudy.
♀ 23	29.973	52.4	29·843	55.3	48	48.9	54.9	46.4	54.9		SSW	A.M. Lightly cloudy. P.M. Fine.
♄ 24	29.858	49.3	29.817	50.5	38	40.8	44.3	36.3	45.2	0.050	NNW	Fine—light clouds and wind.
☉ 25	30.164	46.4	30.196	48.9	35	41.4	46.3	36.7	46.5		N var.	A.M. Cloudless—light haze. P.M. Clear—light clouds.
☽ 26	30.188	45.7	30.134	48.7	41	43.8	48.0	36.7	48.0		WSW	Overcast—hazy.
♂ 27	30.025	47.3	30.025	50.5	45	48.9	51.8	39.3	51.8		WSW	Fine—lightly overcast.
☿ 28	30.117	48.8	30.043	50.7	37	44.2	48.6	39.5	48.3		E	Fine—light clouds and haze.
♃ 29	29.997	45.4	29.911	49.4	33	41.6	52.4	32.4	52.4		NE	Fine and cloudless—light haze and wind.
♀ 30	29.998	46.5	29.924	50.5	40	43.4	51.3	35.4	52.3		N	Fine—light wind.—A.M. Cloudless. P.M. Light clouds.
♄ 31	29.817	46.8	29.750	50.3	42	43.2	48.9	39.7	48.9		NNE	A.M. Overcast—light fog. P.M. Fair—light clouds.
	Mean 29.929	Mean 45.6	Mean 29.887	Mean 48.3	Mean 39.2	Mean 42.2	Mean 47.2	Mean 36.9	Mean 47.7	Sum 0.304		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. …………	9 A.M.	3 P.M.
	29.886	29.835

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge …………………… =83 feet 2½ in.

…………………………………………… above the mean level of the Sea (presumed about) ………… =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ……………………… =79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR APRIL, 1832.

1832. April.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
● ☉ 1	29.842	48.7	29.880	51.4	42	44.8	50.7	41.7	50.7		NNW	A.M. Overcast. P.M. Fine.
☽ 2	30.142	50.6	30.148	53.4	39	46.7	57.2	39.6	57.4		NW	Fine and cloudless—light haze.
♂ 3	30.425	51.4	30.412	55.6	45	49.1	60.8	39.6	63.3		W	Fine and cloudless—faint haze.
☿ 4	30.568	52.6	30.531	57.2	48	50.8	65.0	43.7	65.0		W	Fine—light haze.
♃ 5	30.499	56.3	30.414	59.8	49	53.7	66.3	47.7	67.0		SE	A.M. Strong haze. P.M. Fine.
♀ 6	30.423	54.7	30.372	58.7	49	49.2	55.8	44.3	55.8		NNE	Lightly overcast—light wind.
♄ 7	30.286	50.0	30.162	54.0	40	39.8	53.3	37.4	53.3		NNE	Fine—lightly overcast.
☉ 8	30.176	51.3	30.156	54.7	44	47.3	51.8	39.5	52.7		E	Fine. A.M. Lightly cloudy. P.M. Cloudless—light wind.
☽ 9	30.239	49.3	30.213	54.0	45	45.3	54.3	38.6	54.7		N	Light wind.—A.M. Overcast. P.M. Cloudless.
♂ 10	30.236	47.6	30.162	52.5	41	42.7	55.2	37.4	55.2		N	A.M. Overcast. P.M. Fine—nearly cloudless.
☿ 11	30.196	50.1	30.095	52.9	36	46.3	53.7	36.7	53.7		NE	Fine.—A.M. Cloudless. P.M. Light clouds.
♃ 12	29.954	47.7	29.938	51.8	43	45.0	50.2	38.4	50.2	0.014	ENE	Cloudy.—Rain, early A.M.
♀ 13	29.930	49.7	29.976	54.5	47	47.2	53.2	41.4	54.5		N	Fine—light clouds.
♄ 14	30.164	50.4	30.150	55.6	47	48.3	57.2	39.5	57.2		E	Lightly overcast.
○ ☉ 15	30.136	53.3	30.085	56.6	45	50.0	57.3	43.4	58.3		N	Lightly overcast.—Light rain P.M.
☽ 16	30.036	55.1	30.011	58.4	50	52.6	59.3	48.2	60.8	0.094	WSW	Fine—lightly overcast.
♂ 17	30.048	52.6	29.972	56.0	46	46.5	56.5	41.7	56.7		SW	Lightly overcast—light fog.
☿ 18	29.769	56.5	29.598	58.5	46	53.7	58.9	44.7	59.7		SSE	A.M. Cloudless. P.M. Overcast—rain.
♃ 19	29.646	55.7	29.762	57.2	48	51.3	58.3	46.3	58.3	0.014	W	Cloudy—light wind.—Showery P.M.
♀ 20	29.758	54.7	29.656	56.4	50	51.3	51.9	44.3	52.7	0.094	SSE var.	A.M. Cloudy—light brisk wind. P.M. Light rain.
♄ 21	30.104	55.4	30.160	57.4	44	49.7	57.9	39.7	58.6		NNW	Fine—light wind.—A.M. Cloudless. P.M. Light clouds.
☉ 22	30.196	56.6	30.091	59.0	46	53.2	59.5	43.3	59.7		S	Very clear—light clouds and breeze.
☽ 23	29.817	56.4	29.724	58.9	44	55.0	61.7	46.7	62.6		SSE	Fine—clouds and light wind.
♂ 24	29.740	54.7	29.737	56.5	48	48.0	51.2	47.9	52.4		N	Overcast—light wind.
☿ 25	29.880	52.7	29.883	56.8	46	46.7	53.0	43.4	53.7	0.028	?	A.M. Cloudy. P.M. Fine.
♃ 26	29.766	51.3	29.770	52.3	44	44.7	45.4	42.4	48.7		NW	Overcast—light rain.
♀ 27	29.883	53.4	29.857	54.7	47	49.7	53.2	40.9	53.8	0.097	NNE	Fine—lightly overcast.
♄ 28	29.766	51.7	29.672	54.3	42	46.3	52.5	37.3	52.6		NNE	Fine—lightly cloudy—light wind.
☉ 29	29.547	54.4	29.494	55.8	44	51.2	54.6	39.3	55.3		E	Fine.—A.M. Lightly cloudy. P.M. Clear and cloudless—light wind.
● ☽ 30	29.296	53.3	29.366	57.0	43	52.7	57.0	46.3	58.3		NNE	Cloudy—light brisk wind.
	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Mean	Sum		
	30.016	52.6	29.982	55.7	44.9	48.6	55.8	42.0	56.4	0.341		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. .... { 9 A.M. 29.952 — 3 P.M. 29.909 }

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge........................ =83 feet 2¼ in.
.... above the mean level of the Sea (presumed about) .... =95 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
Height of the Receiver of the Rain Gauge above the Court of Somerset House .... =79 feet 0 in.
The hours of observation are of Mean Time, the day beginning at Midnight.
The Thermometers are graduated by Fahrenheit's Scale.
The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR MAY, 1832.

1832. May.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
						Fahrenheit.		Self-registering.				
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		9 A.M.	3 P.M.	Lowest.	Highest.			
♂ 1	29.433	52.3	29.270	53.5	47	48.3	52.8	42.3	54.7		E	A.M. Rain, with wind. P.M. Fair.
☿ 2	29.335	54.7	29.358	58 9	53	55.2	59.7	46.3	61.6	0.333	S var.	A.M. Clear—cloudy: light rain early. P.M. Fine.
♃ 3	29.459	59.3	29.359	58.8	53	58.3	55.5	49.3	58.6		SSW	Fine—cloudy.—Light rain, P.M.
♀ 4	29.695	54.7	29.857	56.2	48	48.8	57.0	46.6	51.7		E	Overcast.
♄ 5	30.111	54.4	30.047	57.4	51	51.7	56.0	42.4	57.7		SSE	Overcast—light wind and rain.
☉ 6	30.048	57.6	30.068	62.2	57	57.9	65.3	51.7	66.3		SSW	Fair—cloudy.—Light showers, P.M.
☽ 7	29.908	59.3	29.798	64.2	58	58.3	74.0	53.3	74.7		SSW	Fine—light clouds and haze.—Thunder, with heavy rain, early A.M.
♂ 8	30.038	65.0	30.039	64.9	54	58.6	65.5	49.3	67.3		W	Fine.—A.M. Cloudless—hazy. P.M. Light clouds.
☿ 9	30.304	58.8	30.305	58.3	30	48.6	50.9	40.7	51.6		N	Light brisk wind.—A.M. Fine. P.M. Cloudy.
♃ 10	30.414	56.7	30.400	56.0	32	48.2	48.9	39.2	51.0		N	Lightly cloudy.—A.M. Fine and clear. P.M. Lightly overcast.
♀ 11	30.341	54.3	30.196	57.8	43	49.4	55.6	39.3	56.4		NNE	Lightly cloudy.—Light wind, P.M.
♄ 12	29.961	55.4	29.865	56.9	30	46.8	52.0	41.3	52.3	0.069	NNW	Fine—light clouds and wind.
☉ 13	29.712	56.3	29.721	55.7	39	49.3	52.2	41.3	53.7		NNW	Fine and clear—light clouds—showery.
○ ☽ 14	29.803	51.6	29.748	56.1	44	46.3	53.3	39.3	53.6		N	A.M. Lowering—light wind. P.M. Fair—light clouds.
♂ 15	29.784	51.3	29.770	55.5	42	46.6	52.0	40.4	55.2		E	A.M. Cloudy—light wind. P.M. Thunder, with hail and light rain.
☿ 16	29.885	56.7	29.810	56.8	46	49.3	55.8	38.4	56.3	0.111	N	A.M. Cloudy. P.M. Fine.
♃ 17	29.852	50.3	29.887	55.8	46	46.7	56.6	40.9	56.6	0.097	N	Frequent showers. Dark and lowering, with heavy rain at 5 P.M.
♀ 18	30.111	56.8	30.126	59.0	48	54.2	59.3	43.7	60.7	0.278	E	Fine.—A.M. Lightly cloudy. P.M. Fine and clear.
♄ 19	30.252	61.2	30.214	61.3	46	57.3	63.0	43.8	65.7		SSW	Fine and cloudless.—Hazy A.M.
☉ 20	30.217	64.4	30.202	63.9	52	60.7	62.6	45.3	64.3		E	Clear and cloudless—light haze and wind.
☽ 21	30.245	60.8	30.216	63.4	49	58.3	65.5	47.7	66.7		ENE	Fine—lightly cloudy.
♂ 22	30.258	67.4	30.206	66.9	54	60.2	66.2	51.3	67.7		WSW	A.M. Fine and cloudless. P.M. Fair—lightly overcast.
☿ 23	30.305	69.4	30.285	67.8	52	63.6	66.3	50.8	69.3		NNW	Fine.—A.M. Cloudless. P.M. Light clouds.
♃ 24	30.317	63.7	30.265	66.3	57	59.7	68.8	51.7	69.4		E	Fine—lightly cloudy.
♀ 25	30.219	65.3	30.150	67.9	59	62.3	70.3	52.3	70.8		WSW	Fine—haze and light clouds.
♄ 26	30.019	66.3	29.986	68.7	58	63.6	67.3	55.7	68.7		NNW	A.M. Fair. P.M. Cloudy.
☉ 27	30.029	69.7	29.983	66.9	46	61.7	63.2	48.5	65.3		E	Fine—light clouds.—Clear, P.M.
☽ 28	29.964	69.6	29.917	68.7	53	64.3	69.0	45.0	70.6		WSW	Fine—light clouds.
● ♂ 29	29.913	62.4	29.900	65.4	52	57.1	65.0	54.3	66.3		SW	A.M. Overcast—light deposition. P.M. Fine and cloudless. Evening, showery.
☿ 30	29.924	65.9	29.829	66.3	58	60.0	66.2	50.3	67.4	0.153	ESE	A.M. Overcast. P.M. Fine and cloudless.
♃ 31	29.506	62.3	29.454	63.8	56	56.9	58.8	52.7	59.4	0.131	SSE	A.M. Rain. P.M. Overcast.
	Mean 29.979	Mean 59.8	Mean 29.943	Mean 61.3	Mean 48.8	Mean 55.1	Mean 60·5	Mean 46.3	Mean 61.6	Sum 1.172		

Monthly Mean of the Barometer, corrected for Capillarity and reduced to 32° Fahr. …………	9 A.M.	3 P.M.
	29.894	29.853

OBSERVANDA.

Height of the Cistern of the Barometer above a Fixed Mark on Waterloo Bridge…………………… =83 feet 2¼ in.

………………………………… above the mean level of the Sea (presumed about) ………… =95 feet.

The External Thermometer is 2 feet higher than the Barometer Cistern.

Height of the Receiver of the Rain Gauge above the Court of Somerset House ………………… =79 feet 0 in.

The hours of observation are of Mean Time, the day beginning at Midnight.

The Thermometers are graduated by Fahrenheit's Scale.

The Barometer is divided into inches and decimals.

METEOROLOGICAL JOURNAL FOR JUNE, 1832.

1832. June.	9 o'clock, A.M.		3 o'clock, P.M.		Dew Point at 9 A.M. in degrees of Fahr.	External Thermometer.				Rain, in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	Remarks.
	Barom.	Attach. Therm.	Barom.	Attach. Therm.		Fahrenheit.		Self-registering.				
						9 A.M.	3 P.M.	Lowest.	Highest.			
♀ 1	29.609	61.2	29.655	62.8	55	54.9	59.3	50.8	59.7	0.041	WSW	A.M. Rain—light fog. P.M. Overcast.
♄ 2	29.871	63.9	29.857	65.3	55	59.2	65.8	46.8	66.7		NW	A.M. Fair—lightly overcast. P.M. Fine—light clouds.
☉ 3	29.760	67.8	29.656	67.1	56	61.4	69.6	51.7	70.3		NNE	A.M. Fine and clear—faint haze. P.M. Fair—cloudy.
☽ 4	29.549	65.5	29.501	64.8	59	58.9	60.8	55.3	61.7		E	A.M. Cloudy. P.M. Light rain.
♂ 5	29.512	64.4	29.553	66.6	52	61.7	64.2	54.7	65.1	0.408	S	Fine—cloudy.—Light rain, early A.M.
☿ 6	29.489	65.7	29.479	66.4	53	62.6	64.0	51.7	67.3		SSE	Fine—light haze.—Evening, clear—lowering.
♃ 7	29.584	64.4	29.651	64.5	54	62.3	61.0	52.3	66.7		SSW	A.M. Fine—cloudy. P.M. Overcast. Thunder-storm at 11 A.M. and at 1½ P.M.
♀ 8	29.718	61.3	29.739	65.6	56	56.9	63.8	52.9	65.1	0.514	E	A.M. Overcast. P.M. Fine—light clouds.
♄ 9	29.859	66.3	29.847	67.4	55	62.7	60.8	54.5	69.4	0.019	SSW	A.M. Cloudy. P.M. Violent thunder-storm from 3 to 4 o'clock.
☉ 10	29.909	62.3	29.896	66.7	57	58.8	68.1	53.7	68.7	0.350	WSW	A.M. Lowering. P.M. Fine and clear—cloudy.
☽ 11	29.810	63.5	29.749	67.2	59	60.2	65.2	55.8	67.7		E	Overcast—light rain
♂ 12	29.586	66.5	29.531	69.2	66	67.1	69.9	59.8	71.8	0.333	E	Cloudy—light wind.—Heavy rain early A.M.
○ ☿ 13	29.570	76.3	29.540	71.8	63	70.8	70.0	59.8	72.7		SSE	Fine—light clouds.—A.M. Clear. P.M. Showery.
♃ 14	29.695	70.6	29.746	70.8	63	66.4	67.8	57.8	71:0		SSW	A.M. Fine—cloudy. P.M. Overcast—light shower.
♀ 15	29.991	73.8	30.006	69.0	55	65.4	68.0	56.3	70.2	0.028	WSW	A.M. Fine—cloudy. P.M. Heavy showers.
♄ 16	30.104	66.2	30.095	68.6	60	62.4	67.9	53.3	69.4	0.367	W	Fine—lightly overcast.
☉ 17	30.108	65.7	30.103	69.6	61	62.8	70.3	58.7	73.3		SW	A.M. Overcast. P.M. Fine—cloudy.
☽ 18	30.177	75.8	30.136	72.2	61	69.4	75.4	57.7	76.2		SW	Fine—lightly cloudy.
♂ 19	30.098	69.7	30.090	72.5	56	64.6	71.4	59.4	74.3		SSW	Fine—lightly overcast and hazy.
☿ 20	30.029	72.2	29.976	71.4	...	68.4	68.9	65.0	73.3		NW	A.M. Fine—light clouds and haze. P.M. Overcast—heavy rain at 1½ h.
♃ 21	29.985	73.4	29.802	71.3	63	62.9	65.8	55.7	71.3	0.353	N	Cloudy—light brisk wind.
♀ 22	29.531	66.7	29.560	69.5	61	61.5	64.2	56.4	66.4	0.355	S	Overcast.—Rain at ½ past 10 A.M.
♄ 23	29.848	72.5	29.835	70.7	61	64.3	71.0	53.4	72.0	0.039	W	A.M. Cloudy. P.M. Fine—light clouds.
☉ 24	29.981	75.8	29.978	70.8	57	65.5	65.8	52.3	67.5		SSW	Fine—light clouds and wind.
☽ 25	30.039	72.9	30.057	69.5	57	61.6	66.5	52.2	66.7	0.036	SW	Light wind.—A.M. Overcast. P.M. Fine.
♂ 26	30.163	71.6	30.161	68.9	51	60.1	64.3	50.8	65.6		N	A.M. Cloudy. P.M. Fine—light clouds.
☿ 27	30.301	71.3	30.297	69.8	53	63.6	70.6	50.3	71.4		N	Fine—lightly cloudy.
● ♃ 28	30.402	66.7	30.374	70.2	63	65.7	74.0	60.7	74.7		N	Overcast.
♀ 29	30.439	76.4	30.395	73.6	59	69.8	75.2	59.4	76.7		NNE	Fine—light clouds.
♄ 30	30.448	69.3	30.423	71.5	60	64.2	69.0	58.3	70.7		ENE	Fine and cloudless.
	Mean 29.905	Mean 66.5	Mean 29.890	Mean 68.8	Mean 58.0	Mean 63.2	Mean 67.3	Mean 55.2	Mean 69.5	Sum 2.843		

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